

THE MATHEMATICAL GAZETTE

EDITED BY
W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF
F. S. MACAULAY, M.A., D.Sc.

AND
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GEOMETRY AND RELATIVITY.

BY PROF. H. T. H. PIAGGIO, D.Sc.

Thus Euclidean geometry does not hold in the gravitational field even in the first approximation, if we conceive that one and the same rod independent of its position and orientation can serve as the measure of the same extension.

THIS quotation from Einstein is merely one of the many disturbing statements that a teacher of geometry may find in recent books on physics or even in the newspapers. It will not be long before some of our pupils will ask if it is worth while to continue the study of a subject of which the principal results are apparently contradicted by the latest discoveries. The object of the present article is to examine these apparent contradictions. To do this we shall trace the evolution of ideas concerning the nature of geometry and consider their relation to school work. The conclusion will be reached that certain changes in our teaching are desirable. Fortunately these changes will actually make school geometry easier as well as more scientific.

The traditional idea of geometry was that it was an infallible science deduced by strictly logical reasoning from exact definitions and self-evident axioms and postulates, and at the same time a physical science giving relations between measurements of distances and angles made with measuring rods, protractors, sextants, theodolites, and other material bodies. But a few, even from the time of Proclus (A.D. 5), considered the treatment of parallels a blemish in an otherwise perfect science. Euclid's fifth postulate was: *If a straight line falling on two straight lines make the interior angles on the same side less than two right angles, the two straight lines, if produced indefinitely, meet on that side on which are the angles less than two right angles.* Unlike the other axioms and postulates, which appeared self-evident, the fifth postulate was of a complicated character. An improvement was made by replacing it by the so-called Playfair's axiom (really due to Ludlam, 1785) that *two intersecting straight lines cannot both be parallel to the same straight line.* But even this cannot be called self-evident. For, consider a straight line l and a point O outside it. Draw ON , the perpendicular from O on to l , and take any point P on l . If P moves away from N to infinity, OP tends to a definite limiting position OL . We get two such limiting positions OL and OL' corresponding to the two portions into which N divides the line l . Can we be sure that LO and OL' will lie in the same straight line? Many attempts were made to prove this by a *reductio ad absurdum*, but none succeeded. Indeed Bolyai (1823) and Lobachevsky (about 1826) built up a system of geometry by

denying this axiom, while retaining the other axioms and postulates. This system seemed perfectly consistent in itself, and finally Klein (1871) proved rigorously that no mutually contradictory results could possibly occur. However, nearly everyone looked upon this kind of geometry as merely a logical curiosity, without application to the real world. Those who concerned themselves with such speculations were regarded as cranks, if not as lunatics. Gauss, whose work on the subject preceded that of Bolyai and Lobachevsky, so greatly feared "the outcry of the Bæotians" that he kept his researches secret. Eventually the most enlightened mathematicians conceded that from a purely abstract standpoint two systems of geometry were equally possible.

In 1854 Riemann introduced a third system, in which parallel lines do not exist at all! Now this is clearly contrary to our ideas of straight lines. With the notation that we have used above, consider a line through O perpendicular to ON . If the line through O is not parallel to l , it must meet it, say in M . Then by the symmetry of the figure the lines must meet in another point M' which is such that N is midway between M and M' , and the two straight lines MOM' and l enclose a space. This is contrary to one of Euclid's axioms. Very well, said Riemann, we will reject that axiom!

At this point most teachers of geometry will declare that Riemann was talking nonsense, or at least using words in an illegitimate sense. What he called a straight line was of finite length, like a great circle on a sphere. This is certainly in contradiction to our ordinary ideas, but it is *not* opposed to our definition of a straight line, as a line which *lies evenly between its extreme points*. As a matter of fact this definition (which is never used in proving our propositions) has no meaning unless it is supplemented by directions for testing the evenness mentioned. The usual test is to put two rulers together and rotate them. All the points along the edges will be found to remain more or less in contact. With school rulers the contact will probably be less rather than more, but this will be attributed to the warping of the wood. Our definition thus assumes that we know what is meant by a rigid unwarpage body. A rigid body is one such that the distance between any two points remains constant. To test this constancy we measure the distance, but to do so we use a ruler which is itself assumed rigid. It is evident that we are merely working in a circle. Somewhere we come to a term which cannot be defined. Of course we may say that everyone knows what a rigid ruler is, even though we cannot define it or make a material one which fulfils our ideas entirely. We are quite entitled to build up our geometry on such a basis, as Pieri (1899) has done, but we have no right to object to Einstein building up a different system of geometry (Riemannian) on the basis of different ideas concerning the behaviour of rulers, which he conceives to be slightly affected by the neighbourhood of heavy masses. If a straight line be defined by the motion of these rulers, or defined as the shortest distance between two points as measured by these rulers, there is no obvious reason why two such "straight lines" should not enclose a space.

When the reader has recovered from the shock of finding the use that Riemann and Einstein have made of the vagueness in the definition of a straight line, he may be prepared to learn that Hilbert and others leave this term and others without any definition at all! This, however, is really inevitable, as all definitions must define one term by others, so ultimately there must be at least one term which cannot be so defined. Hilbert's *Foundations of Geometry* (1899) contained a set of undefined terms, such as *point*, *straight line*, *plane*, and another set of axioms containing statements concerning the relations between these terms. These axioms are not taken as self-evident. What is more, they are not even taken as necessarily true (in any physical sense). All that Hilbert implies is that he is going to play a sort of logical game, which we will call *abstract geometry*, starting with a certain stock of

undefined terms and unproved propositions (axioms) concerning these terms, and deducing other propositions from these unproved ones. It may be noted that Hilbert's list of axioms is much longer than Euclid's. This is because Euclid often reasoned from the figure or made unconscious assumptions. However, Hilbert's work is really the natural development of Euclid's. But it is now clear that instead of *one* set of definitions and axioms, we may start with many different sets. The only requirement is that each set should be self-consistent. Each set will lead to a corresponding system of abstract geometry. Of such systems we have already distinguished three, Euclid's, Lobachevsky's, and Riemann's.

But let us return to the real universe. Many will agree with Klein that "to regard the objects of mathematical study as mere empty symbols sounds the death knell of all science." Cannot we dismiss all these new geometries as physically untrue, or at least unnecessary? Let us make a careful drawing and measure the three angles of a triangle. Better still, let us measure the angles of a triangle whose vertices are three mountain tops. This was actually done by Gauss, who found that the sum of the angles differed from two right angles by an amount so small that it could be attributed entirely to experimental error. It is certain that the most careful experiments as yet made show that *physical geometry* agrees with Euclidean abstract geometry within the limits of experimental error. But will they always agree, as measurements grow more and more refined? Poincaré was of opinion that this agreement could always be maintained. Our experiments do not test geometry alone, but the combination of geometry and physics. For instance, in using optical instruments we assume that light travels in straight lines. If Gauss's experiment had given a different result, he might have retained his faith in Euclid by assuming that the light rays were really slightly curved. If our drawing disagrees with our Euclidean notions, we may say that our rulers have undergone some physical change. Poincaré (1905) asserted that any system of abstract geometry could be combined with a suitable system of physical assumptions to agree with all the results of experiment, but that it would always be found most convenient to retain Euclidean geometry and introduce our modifications entirely into our physics. But Einstein (1921) thinks that the behaviour of graduated rulers and of rays of light is more conveniently described in terms of Riemann's geometry and relativity physics, rather than in Euclidean geometry and another system of physics more complicated than even relativity. Weyl (1920) goes further than Einstein in disagreeing with Poincaré, and he sees no reason why experiment should not some day be able to decide between Euclidean and Riemannian geometry, provided that all physics is worked out on both bases.

It is hoped that the quotation with which we started is now intelligible. It is a statement of a physical nature. The same may be said of the further statement of relativitists that "a straight line produced indefinitely may return to itself." By a *straight line* is here meant the *path of a ray of light*. It is only fair to point out that Einstein's ideas on geometry have not been accepted universally. Painlevé (1921) has given alternative forms of relativity theory which agree with Einstein's in those portions which can be tested by observation, and yet disagree altogether as to the contractions of measuring rods in a gravitational field. If Painlevé's views can be sustained, the preference of mathematical physicists for Riemannian geometry may be only a passing phase. On the other hand, Weyl's extended theory of relativity (1918) makes the length of a rod dependent upon all the past history of its movements near electric currents! His system of geometry is a fearful and wonderful thing, compared with which Riemann's is comparatively simple.

Let us console ourselves that Einstein said (1920) "I do not believe that his theory will hold its ground in relation to reality."

We shall now consider what aspects of geometry are suitable for schools.

There is no doubt that the great majority of boys cannot assimilate modern ideas on abstract geometry in any form. What we want to teach is physical geometry, on the simplest set of assumptions, namely the Euclidean set. But our consideration of abstract logic will not have been in vain if it has taught us that much of what appears in our ordinary text-books can be omitted with advantage. Consider for instance such a proposition as that concerning the equality of the angles at the base of an isosceles triangle. The boy says that the equality is obvious. The teacher insists that it must be proved, and the proof he requires probably uses a theorem which has been treated by the vicious superposition method.* Now there are two reasonable points of view, that of the boy, who relies whole-heartedly on intuition, and that of Hilbert, who tabulates a long list of axioms and reasons from these alone. The teacher who requires elaborate proofs which are repugnant to a boy's intuition in some places, and equally repugnant to Hilbertian logic in others, is combining the disadvantages of both methods. Many generations of schoolboys have groaned under the burden of "proving the obvious." This leads to a sense of unreality and a widespread distaste for geometry. One recent text-book† has had the courage to discard these obvious propositions, thus reducing the number that have to be formally studied by about half. This book treats congruence and parallelism as axiomatic notions, to be led up to by drawing. It is too soon to say definitely whether this treatment is the best, or whether we should follow Prof. Nunn, who takes as his axioms:

- (1) *A given figure can be exactly reproduced anywhere ;*
- (2) *A given figure can be reproduced anywhere on any (enlarged or diminished) scale.*

In either case it ought to be stated that these axioms are assumptions, and that many other assumptions are made throughout the treatment, the justification for these being their excellent agreement with drawing, surveying, and scientific measurement generally. Possibly the last page of the book might contain a brief note as to the possibility of other systems of geometry, with a list of the most elementary introductions to the subject.‡ Many who are repelled by the usual presentation of school geometry, as a cold perfection to which nothing can ever be added or taken away, will be glad to learn that our science is really a living and growing thing, with many human weaknesses to overcome, but ever striving to know more of the truth.

H. T. H. PIAGGIO.

GLEANINGS FAR AND NEAR.

126. This evening I was with my Lord Brouncker, Sir Robert Murray, Sir Pa. Neill, Monsieur Zulichem, and Mr. Bull (all of them of our Society and excellent mathematicians), to shew his Majestie, who was there present, Saturn's annulus, as some thought, but as Zulichem affirm'd with his Balleus (as that learned gentleman had publish'd), very near eclipsed by the Moon. . . .—*Evelyn's Diary*, May 3, 1661.

[This is "old Monsieur Zulichem, Secretary to the Prince of Orange, . . . a rare Latinist, now neare 80 yeares of age" (1664).]

* For the objections to this see Prof. Nunn's article in the May number of the *Gazette*.
† *Plane Geometry*, by V. Le Neve Foster (Bell).

‡ Such as J. W. Young's *Fundamental Concepts of Algebra and Geometry* (Macmillan).
A. Einstein's *Sidelights on Relativity* (Methuen).
D. Hilbert's *Foundations of Geometry* (Open Court).
H. S. Carslaw's *Non-Euclidean Geometry and Trigonometry* (Longmans).
D. M. Y. Sommerville's *Non-Euclidean Geometry* (Bell).

TWO EXAMPLES OF EXPERIMENTAL RESEARCH IN CON-
NEXION WITH THE TEACHING OF ARITHMETIC.

BY MISS MARGARET PUNNETT, B.A.

THERE can be few teachers who, when taking part in a discussion on the relative merits of two methods of teaching a given topic, have not wished that it were possible to produce in evidence a clear, accurate and unbiassed account by the child himself of the way in which the particular method under discussion affects him. As it is, we are forced to rely on the experience and insight of the teacher—experience, which is at best very limited, and insight which, if it is often true and sympathetic, can never fully penetrate into the child mind, and is moreover scarcely ever free from a prejudice in favour of one method or the other.

What is needed in these and other questions is evidently a much more frequent use of psychological experiment and research, and it is greatly to be hoped that investigations of this kind will be freely used in the not too distant future to throw light on the various vexed questions in the teaching of mathematics. Meantime teachers will obviously be well advised to consider carefully those investigations which have already been undertaken and, where possible, to act upon their results.

Two such pieces of research have been carried out during recent years by Mr. W. H. Winch, one an investigation into the comparative merits of the methods of "decomposition" and "equal additions" in connexion with subtraction, and the other an inquiry into the age at which the teaching of arithmetical proportion should be begun.

I. **"Equal Additions" versus "Decomposition" in teaching Subtraction.*

All teachers of young children have at some time—if not at many times—been concerned with the question of the comparative merits of these two methods. For the sake of those who are not familiar with these quasi-technical terms, it may be well, perhaps, to illustrate them by an example :

$$\begin{array}{r} 73 \\ -28 \\ \hline 45 \end{array}$$

Method of Decomposition.—Since 8 cannot be taken from 3, take 1 ten from the 7 and subtract 8 from it. 8 from 10 leaves 2, which, added to 3, gives 5 in the units' place. Then 2 tens taken from 6 tens leave 4 tens.

Method of Equal Additions.—Since 8 cannot be taken from 3, add 10 to the upper line. Then, as before, 8 from 10 leaves 2, which, with 3, makes 5. As 10 has been added to the upper line it must be added also to the lower line ; hence we complete the work by taking 3 tens from 7 tens, leaving 4.

The chief argument used by the advocates of the method of decomposition is that it is the natural and obvious one for the child, and appeals to him as the most reasonable way of meeting the difficulty. The advocates of the method of equal additions on the other hand argue that, although it is a little more difficult to understand than the other method, it is not insuperably difficult, while it has the great advantage of leading to more accurate and rapid working, especially in cases in which there is a row of zeros in the upper line. Mr. Winch's experiments have aimed at testing whether or not this opinion as to the better results of the method of equal additions is justified.

There are two main lines on which such an investigation may proceed. One is to take a large number of children, some taught by one method, some by the other, and test them carefully as regards both the accuracy and rapidity

* The titles quoted are Mr. Winch's own.

of their working of tests in subtraction. The drawback of this method lies in the difficulty of eliminating elements, other than the method of working, which may affect the result of the teaching. The skill of the teacher, his conviction as to the superiority—or otherwise—of the method he is using, the conditions under which the children work at school, their home conditions and the effect of these on their physique, general well-being and intelligence, all evidently have their share in the results produced by any method. Theoretically the effect of these extraneous factors can be eliminated by taking such a large number of cases under such a variety of conditions that differences balance one another. It is obviously difficult to carry this out. Some years ago Dr. Ballard conducted an experiment of this kind to investigate the same problem that Mr. Winch proposed to himself, namely to ascertain the comparative merits of the two methods of working subtraction. The results of Dr. Ballard's investigation, though not very striking, were such as to suggest that, if the experiment could have been extended over a wider area, the result would have been to demonstrate the superior results obtained when the method of equal additions is used.

Mr. Winch's method was very different from Dr. Ballard's. He experimented with a small number of children only—not more than about forty in each case. The children in each experiment were chosen from the same school, so that they were working under the same conditions; they were of about the same age. They were given a preliminary test which was used as a means of dividing them into two equal and parallel groups of approximately equal average age and equal average attainments in subtraction, and one of the groups was then taught by one method and the other by the second.

Two experiments were carried out by Mr. Winch under these conditions, one with girls of average age 11 years 11 months, the other with girls of average age 8½ years. The girls were pupils respectively of two County Council elementary schools in south-west London.

The elder of the two sets of girls, almost all of whom had been accustomed to work by the method of decomposition, were divided, in the way already described, into two equal and parallel groups, A and B. Group A was given a series of lessons in the method of equal additions, and was practised in working subtraction by that method instead of the one (decomposition) with which they were familiar. A certain amount of confusion was naturally experienced at first in passing from one method to the other, but this was soon overcome. Group B was given an equal number of lessons in the method of decomposition, with which they were already accustomed to work, the object of these lessons being to remove any weaknesses and confusions shown by the girls, and to give them practice in working difficult as well as simple examples as readily and neatly as possible. After ten lessons had been given to each group, both were tested by the same series of examples in subtraction, group A working by the new method of equal additions, group B continuing, of course, in the method of decomposition.

In spite of the fact that in the case of group A the method of equal additions was superimposed, as it were, on the method of decomposition, the girls of that group showed a distinct superiority over those of group B. (For the details of the results and the actual figures, as well as the correlation-coefficients and the "probable errors" calculated from them, the reader is referred to Mr. Winch's own account of his experiments in the *Journal of Experimental Pedagogy*,* volume 5, nos. 5 and 6.) This result will be admitted to be strong evidence in favour of the method of equal additions. At the same time Mr. Winch gives it as his opinion that the superiority of the girls of group A over those of group B was not sufficient to justify the expenditure of time and effort necessary to change the method at that age.

* Published by Longmans, Green & Co. at 1s. 6d. per number.

The second experiment was in the main a repetition of the first, the only differences being those due to the fact that the girls experimented upon were younger, and showed consequently in the first instance much more variety in their attainments as regards subtraction. Some of them had been already taught to use the method of decomposition, some to use equal additions, while a considerable number had learned neither method. These girls, too, were divided into two equal and parallel groups, and each group, as in the first experiment, received a series of lessons (in this case eight in number), group A in the method of equal additions, and group B in that of decomposition. Here as before the superiority of the method of equal additions, as tested at the end of the experiment, by the accuracy and rapidity with which examples in subtraction were worked, was clearly marked.

It may, therefore, fairly be taken as proved that the method of equal additions produces results better, even if only slightly better, than the method of decomposition.

If this conclusion is accepted, it would naturally follow that the method of equal additions should, from now onwards, be the method adopted in schools unless it could be shown that that method has drawbacks sufficiently serious to counterbalance its advantages. Some teachers maintain that it has such drawbacks. It is, they argue, dangerous to accept the principle that the method which produces the best practical results is necessarily the one to be chosen in teaching. We must aim not only, or even chiefly, at making our pupils quick and accurate computers, but also at developing their intelligence by leading them to use methods which they can see to be reasonable. If a method is so difficult that children cannot use it intelligently we will have none of it, even if it produces results in practice which are far superior to those of any other method.

We should indeed be on the horns of a painful dilemma if we were forced to choose between these two alternatives: a simple, sound and reasonable method with poor results on the one hand, and on the other a difficult method beyond the understanding of the children but producing excellent results. Fortunately there is no such dilemma here. The method of decomposition is undoubtedly—at any rate in the early simple cases—easier and more obvious to children than the method of equal additions. But teachers who favour the former method tend much to overrate the difficulty of the method of equal additions. Even if it is based upon the abstract and therefore difficult axiom that, if we add the same quantity to each of two unequal ones, the difference between these two will be the same as before, the axiom can be introduced and freely illustrated by examples, and though difficult it proves by no means impossible of grasp by children. (It may, perhaps, be permissible to whisper the suggestion that, even if some children were obliged to take the axiom for granted at this early stage, no great harm would be done.) But, as most teachers of the method of equal additions know, it is possible to present the principle in a slightly different form which makes it much easier for the child mind to grasp it. Taking the example of subtraction already used:

$$\begin{array}{r} 73 \\ -28 \\ \hline \end{array}$$

If, in order to overcome the difficulty which arises from the fact that 8 is greater than 3, we choose to add ten to the 73, it is clear that the final answer will be too big by ten, unless we presently take ten away again. We can most conveniently do this at the same time that we take the 2 from the 7, that is, we will take 3 from 7, leaving 4. With the aid of sticks, beans, or other material, even dull children find no great difficulty in following this argument. The only difficulty that will arise will be the objection which most intelligent children at once make to this method: "Why should you add another ten when you already have 7 tens lying there ready for use?" In the light of Mr. Winch's experiments the answer is quite plain. The teacher who accepts

his result—and it is difficult not to accept it—will reply, “Because you will be able to subtract more quickly and correctly if you do it in that way.”

In conclusion, then, it appears that there is no serious drawback to the use of the method of equal additions which could justify us in ignoring the superiority of its practical results.

II. Should young children be taught arithmetical proportion?

This investigation, whose aim is sufficiently indicated by the title, is evidently of an entirely different kind from the one just described. Here it is not a case of comparing two different methods of dealing with the same topic, but of testing the effect of introducing a certain piece of work at certain given points in the course.

In this case Mr. Winch grouped a number of exercises into six “steps,” which were worked out by sets of children under various conditions. The steps were as follows:

Step I. Simple questions in proportion which require “unquantified” answers, that is, answers which merely distinguish between “more” and “less.”

E.g. I paid 2s. for 2 pounds of butter. Shall I have to pay more or less for 4 pounds of butter?

A man uses 16 nails to make 2 boxes. Will he want more nails or less nails to make 9 boxes?

Step II. Questions similar to those of Step I., but requiring the child to move mentally to or from unity.

E.g. I pay 4s. for 4 pounds of butter. Shall I have to pay more or less for 1 pound of butter?

It takes 9 yards of rope to make 1 clothes-line. Will it take more or less yards of rope to make 4 clothes-lines?

Step III. Problems of the same type as in Step I., but requiring numerical answers.

Step IV. Problems of the same type as in Step II., but requiring numerical answers.

Step V. Proportion problems of the usual type presented in such a way that the questions lead to solution by unitary method.

E.g. I pay 4s. for 2 pairs of boots. What shall I pay for 3 pairs?

(1) What shall I pay for 1 pair?

(2) What shall I pay for 3 pairs?

Step VI. Problems of the same kind as in Step V., but presented without the help given by analysing them as in Step V.

The problems were mainly those involving direct proportion, but a problem involving inverse proportion occurs now and then. *E.g.* One of the problems of Step VI. is: If it takes 6 women 2 days to make some clothes, how long will it take 3 women?

Each exercise contained 10 questions, and the number of exercises was from 18 to 20, varying slightly in the different experiments. Steps I., II. and III., consisted of one exercise each. Steps IV., V. and VI. of from four to six exercises each.

The whole series of exercises was worked through with the following groups of children:

1. A class of boys and girls, of average age 7 years and 2 months, in a London infants' school.

2. The whole of a London junior boys' school, consisting of four classes of average ages 7 years 5 months, 8 years 8 months, 9 years 9 months, and 10 years 11 months, respectively.

3. A class of girls, of average age 8 years 9 months, in a London elementary school.

4. All the classes, nine in number, of a London elementary school for girls, the average ages of the classes ranging from 8 years to 13 years 1 month.

The schools were selected in such a way as to give the greatest possible variety of conditions as regards the home circumstances of the children, their general intelligence, and so on.

That period over which the tests were spread varied from 12 to 20 weeks, according to whether they were given twice or only once a week.

Mr. Winch lays stress on the fact that neither before nor during the period in which the exercises were taken was any direct teaching of proportion given to the children.

For the details of these experiments, the tabulated results of the exercises, the consideration of their statistical value and the discussion of many interesting side questions, the reader is again referred to Mr. Winch's own account of the work, published in volume 2, nos. 2, 5, and 6, of the *Journal of Experimental Pedagogy*. It is impossible here to do more than give a brief summary of the main results and of the conclusions Mr. Winch draws from them.

The exercises of Steps I. and II., the questions requiring "more" or "less" for answer, were in the main well done by all the children, Step II. proving easier than Step I. The result of Step III. was on the whole poor, as indeed was expected. Its purpose was chiefly to furnish a standard by which the improvement resulting from the succeeding exercises might be measured. Steps IV. and V. have, in fact, been deliberately arranged by Mr. Winch in such a way that they supersede the teacher; the sequence of the exercises themselves teaches the children the unitary method, and the effect of this "indirect teaching," as Mr. Winch describes it, is seen in the improvement in the results of Step VI. as compared with those of Step III. Mr. Winch explains that he has substituted this "indirect" teaching for the usual direct teaching in order to eliminate from his experiment the effect of differences of teaching power and skill in the teachers of the various classes concerned. He remarks in passing that this indirect teaching is probably better than "all except the best" direct teaching; many of us would no doubt echo his opinion without the qualifying phrase. The results of the last three steps varied considerably in the different cases, depending chiefly on the age of the children. Briefly, the results amount to this: that it is possible to teach proportion successfully in this form to children of age about 9 and upwards, the lower age limit naturally varying with the conditions.

The conclusion which Mr. Winch apparently draws from this result is that proportion, in the guise of unitary method, not only *can* but *should* be taught at the age of nine or thereabouts. But is this conclusion justified? It has proved possible to teach a large amount of Latin grammar, and to teach it "successfully," to small boys of eight. But most people now realise that this is by no means a valid reason for doing so. It is, of course, obvious that, before deciding at what point in the course it is desirable to introduce a given piece of work, we must first know at what point it is possible to introduce it. And here, in connexion with proportion, in one of its forms at least, Mr. Winch's experiments give us a clear and therefore valuable answer. But many other considerations must be taken into account before the problem can be finally solved. Even if the topic in question *can* be taught early, it may at that stage involve a disproportionate expenditure of time and energy; or it may be possible only if it is taught by a method which, in view of later work, is undesirable; or, most important of all, it may crowd out work more valuable and important at that stage. Mr. Winch's experiments show that the first of these possible objections to the early introduction of proportion does not apply. The time and effort necessary for reaching the results he records were apparently not excessive. The other two objections, on the other hand, apply markedly in this case.

The method which is evidently the only possible one at this stage, namely

unitary method, will by no means be accepted by all teachers as the best way of dealing with proportion throughout. It is interesting to note that Mr. Winch not only assumes that most teachers will so accept it, but he points out in the course of his comments on his experiment that it is the method "naturally" adopted by the children. It appears that here Mr. Winch has underrated what may be called the teaching efficacy of his exercises; they were, as we have seen, arranged in such a way as to teach the children unitary method very effectively. It is not surprising, therefore, that the children worked by it. But as the chief method of dealing with problems in proportion it is by no means satisfactory; it fails to make clear the essential character of proportion as it can be made clear by comparing directly the ratios of the quantities involved. (The use of the term "ratio" at the outset is, of course, not an essential part of this method.) Teachers who hold this view will deliberately give to unitary method an inconspicuous and unimportant place in the arithmetic course, in order that a habit of dealing with proportion in that way may not be formed to the detriment of the later work. They may at an early stage give simple exercises which can be solved by the direct "common sense" way of comparing ratios (e.g. If 7 buns cost 6d., what will 21 buns cost ?); but they will chiefly be concerned to see that, in the practical work done in the early years, such exercises in measuring, drawing to scale, etc., are included as will familiarise the children with the fundamental nature of proportion and prepare the way for its systematic study later.

The further objection is also a weighty one, namely, that if systematic work in proportion is introduced at the age of nine or ten, it will crowd out work which is more suitable and more valuable for children of that age. This is hardly the occasion for discussing in detail what the nature of that work should be; but it is generally recognised by teachers of mathematics that the later work often suffers seriously from lack of power on the part of the pupils to handle number with skill and ease and to deal intelligently with its applications to the world about them. To encroach on the time which should be spent in securing such a result by attempting to anticipate later parts of the work will lead in the end to less rather than more rapid progress.

Even if, however, we are not able in this case to accept all the conclusions which Mr. Winch draws from his experiment, we have to thank him not only for a clear and definite answer to the question as to the age at which unitary method can be taught, but also for much interesting and illuminating analysis of the children's mental processes in working his tests. All teachers who are interested in the working of children's minds—and it is to be hoped that there are none who are not interested in it—will find it well worth while to study Mr. Winch's own detailed accounts of his experiments. It is impossible to do so without gaining an increased understanding of the child mind, especially as regards early mathematical ideas.

MARGARET PUNNETT.

[Miss Punnett's interesting paper will no doubt be but the preface to many communications from those who have followed such work with interest, and we hope, from some who are themselves engaged in researches of the kind. The following notes may also be of interest:

In 1913, Mr. W. H. Winch, who was at the time an External Member of the Board of Psychological Studies for the University of London, Chairman of the Psychological Research Council of the Teachers' Guild, and Lecturer for the London County Council on Pedagogical Methods in Schools, published through Messrs. Warwick & York, Baltimore, U.S.A., his *Inductive v. Deductive Methods of Teaching: An Experimental Research*. This little volume of less than 150 pp. formed one of a series of "Educational Psychology Monographs." Mr. Winch is described in the Editor's preface as "well known as the first Englishman to bring the technique of experimental and statistical methods to bear on the actual practical problems of the school." However that may be, it will not be easy to find an earlier attempt to decide between the

"conflicting claims of 'inductive' and 'deductive' methods by experimental procedure." The subject of the tests was the acquisition of the power of geometrical definition by pupils who had never learned geometry. The points at issue were four: which method taught the definitions the quicker; after which method did the pupil forget more or less; which method gave birth to the more serious type of errors; and which method gave the greater power of attacking new material?

In *Child Study*, July 1912 (pp. 60-66), Mr. Winch had a paper entitled: "How a Teacher can test the value of his own Methods." In this he lays down the principles on which a class may be divided into two "equal" groups, i.e. into two groups in which the average ability is the same in the particular function to be measured; the principles on which the selection of the consecutive tests is based, and on which the answers are marked; and gives a description of the method adopted in training one group in one way and the other in another.

The British Journal of Psychology, Sept. 1914 (pp. 190-225), contained a further article: "Some New Reasoning Tests suitable for the mental examination of School Children." The aims of the paper are succinctly stated as follows: "First, I hope to show, as indeed should be shown for all tests before they are generally applied, their very high reliability. Secondly, I hope to demonstrate, for all practical purposes, their non-improvability by practice. Thirdly, I hope to outline, from the consideration of the answers, a method by which the development of all logical and mathematical principles can be traced in the growing mind of the child." The last section of this paper, on a method of determining the development of logical principles in school children, is of remarkable interest.

Perhaps this short and inadequate summary of the researches conducted by Mr. Winch may lead to communications from those who have done similar work, to criticisms of what has been done, and to suggestions for future trials along the same or similar lines and on fresh material.

Does any reader doubt his own competence in the making of experiments? Then let him be encouraged by Mr. Winch: "I say that the people who try, and who eliminate the causes of failure (for failure is almost certain at first) one by one, are well on the way to become expert experimentalists."]

EDITOR.

127. "Do read Mathematics. I should think X plus Y at least as amusing as the *Curse of Kehama*, and much more intelligible. Master S.'s poems are, in fact, what parallel lines might be—viz. prolonged *ad infinitum*, without meeting anything half so absurd as themselves."—Byron to Mr. Harness, Dec. 6, 1811.

128. Lord Lyndhurst (Second Wrangler, 1794) must have done some mathematics after he left Cambridge. In a speech in the Lords, April 29, 1858, against a Bill brought in by the Marquis of Westmeath for the suppression of street music, he said: "I recollect very many years ago when I was studying in chambers, having a neighbour who was learning to scrape on the violin. I was at first disposed to complain of my neighbour's innocent pastime as an annoyance: but on a little reflection I said to myself:—'Is it wise in me to object? Let me see if I can stand it without distraction. If I can, what an admirable discipline it will be to me in pursuing my mathematical studies.' After a time I ceased to hear this 'nuisance' as it was called—it made no impression on me."

From the first, Copley showed great aptitude for Mathematics. His memory was tenacious. In late life he fixed a date by remembering that the event occurred when he had just entered on the study of Newton's *Principia* at the University (Trin. Coll.).

NOTES ON "A TREATISE ON THE RECTILINEAR MOTION
AND ROTATION OF BODIES." By GEORGE ATWOOD,
M.A., F.R.S. (Cambridge, 1784.)

By F. G. HALL, M.A.

This is a remarkable book in many respects, and well worthy of the attention of those teachers of mechanics who have not yet made its acquaintance at first hand. Though written in 1784, some of its pages are greatly superior to those in many of the text-books now in use in schools, and one is particularly impressed by the stress laid on experimental verification of the various laws of mechanics, and by the extreme care shown in the planning and execution of the experiments proposed. The following quotations from it and the notes thereon are given, because I feel sure that they will prove of use to those who want Mechanics to become a more interesting subject to their pupils and to stand for something more than what is usually covered by the name "Applied Mathematics." I have added a few parentheses, but only, I hope, where they were necessary.

In his interesting Preface, Atwood says :

"In books of mechanics many experiments have been described by which the equilibrium of the mechanic powers, the composition and resolution of forces, and other statical principles are explained and verified ; but no account is to be found of methods by which the principles of motion may be subjected to decisive and satisfactory trials. An attempt has been made to supply this deficiency in the 7th and 8th sections of this treatise. . . . The numbers set down were the results of measurements and computations made with great care and attention. . . . These experiments and the explanations of them were part of a course of experimental lectures on the principles of natural philosophy read in the University of Cambridge.

"Experiments on the resistances of fluids to solid bodies moving through them have not been very numerous : those which are described in the second volume of the *Principia* are sufficiently decisive to ascertain the agreement between the author's theory and matter of fact ; but the repetition of experiments by which any important truths are verified cannot be thought superfluous : we cannot make too frequent appeals to experience ; theories, however perfect, are never so satisfactory as when they are illustrated by repeated and accurate trials. The experiments on the descent of spherical bodies in water, inserted in Section V., were constructed with great attention to exactness, and the results were noted before the computations were made."

He gives a most interesting and detailed description of his machine, "such an instrument as will subject to experimental examination the properties of the five mechanical quantities, *i.e.* the quantity of matter moved, the constant force which moves it, the space described from rest, the time of description, and the velocity acquired." He does not neglect, as is done in so many artificial examination questions (see below), the motion of the pulley itself, but says :

"When A , B are set in motion by the action of any weight m , the sum $A + B + m$ would constitute the whole mass moved, but for the inertia of the materials which must necessarily be used in the communication of motion."

"As the figures (of the wheels) are wholly irregular, recourse must be had to experiment in order to assign (their) equivalent quantity of matter. A and B were removed. A weight of 30 grains was affixed to a silk line wound round the main wheel, and allowed to descend. It described about $38\frac{1}{2}$ inches in 3 secs. . . . giving equivalent mass 1323 grains or $2\frac{1}{4}$ ozs."

"The time of motion is observed by the beats of a pendulum which vibrates seconds: and the experiments, intended to illustrate the elementary propositions, may be easily so constructed that the time of motion shall be a whole number of seconds; the estimation of the time therefore admits of considerable exactness, provided the observer take care to let the bottom of the box begin its descent precisely at any beat of the pendulum; then the coincidence of the stroke of the box against the stage and the beat of the pendulum at the end of the time of motion will show how nearly the experiment and the theory agree together."

He shows that the weight of the string will not affect the experiment, calculating that corresponding to a time of 3.9896 seconds when the weight of the string is neglected the corrected time would be 4.0208 seconds. He also says:

"The air's resistance can never increase the time of descent in so great a proportion as 240 : 241"; and

"The effects of friction are almost wholly removed by the friction wheels. 2 grains added to whole mass of 63 quarter-ozs. will communicate motion."

In discussing experiments such as those in which Galileo investigated the motion of balls down inclined planes, he says:

"It is manifest how much experiment will disagree with the theory of the descent of bodies along planes, unless their rotation be taken into account."

He had already proved the proposition: "The force which accelerates the centre of gravity of a sphere while it rolls down an inclined plane is to the force by which it would be accelerated were it to slide in the ratio 5 : 7. . . . For a cylinder 2 : 3."

He then shows how his machine may be adapted to experiments on the rotation of bodies, one proposition which he proves and verifies being as follows:

"In the wheel and axle (used), the radius of the wheel is to the radius of the axle as 10 : 1; the distance of the centre of gyration from the axis is $6.7345 \times$ radius of the axle; the wheel's weight is 6.0635 ozs.

"Suspend A from the axle and B from the wheel, A being 10 ozs. and B 1 oz. Add $\frac{1}{10}$ oz. to B . Then B will descend 19.54 inches in two seconds."

In considering the radii of some of the cylinders used in such experiments, he seems to carry his passion for accuracy rather too far:

"In order to obviate this difficulty (i.e. errors in measuring small radii), the following indirect method was used by which the radii of the cylinders used in the experiments were obtained to very great exactness. Having fixed to the extremity of a very fine and flexible line a weight sufficient to keep the line stretched, fasten the other extremity to the axle of which the radius is required; the line being stretched by the weight mentioned above, measure by a scale of even parts any convenient length, 6 inches for example, and mark the extremities of the length so measured; then, holding the axle horizontal, let the measured part of the line be wound round it in the form of a helix, the circumferences being everywhere contiguous. Count the number of revolutions, and suppose them $=n$, also measure the length of the cylinder occupied by the helix; let this $=a$, and the length of the helix or line first mentioned $=l$, $p=3.14159$ etc.; then the radius of the cylinder $=\sqrt{l^2 - a^2}/(2pn)$. The exactness of this method may be known by observing that if a cylinder be truly made, and the process carefully repeated with different values of l , n , a , the radius deduced will however always come out the same to the 4th or even the 5th decimal place."

After reading the book one wonders whether "modern" methods of teaching mechanics are really so very much better than those actually employed by George Atwood in the year 1784! The following questions, some from modern text-books, the others from recent examination papers, are quoted to illustrate this point:

(1) Two masses m and m' ($m > m'$) are connected by a string passing over a smooth pulley; they are released from rest, and after t seconds the string breaks just as the masses are passing each other. Prove that, if m strikes the ground after t' seconds more, the mass m' will be at a height

$$2(m - m')g \cdot t \cdot t' / (m + m') \text{ feet}$$

above it.

(2) The masses in an Atwood's machine are 40 and 45 grams. Find from first principles the acceleration of the system and the tension in the string. If the string breaks 2 secs. after the weights begin to move, find the position of each of the weights one second later.

(3) A heavy string hangs at rest over a small smooth fixed peg. Show that if $\frac{1}{10}$ ths of the string on one side be cut off, the pressure on the peg is instantaneously reduced to $\frac{1}{10}$ th of its previous amount.

(4) Two weights, each of 4 lbs., are connected by a string which passes over a smooth pulley. A weight of $\frac{1}{2}$ lb. falls from a height of 1 ft. on to one of the equal weights and sticks to it. With what velocity does the system begin to move?

Also one from an older book still frequently used:

(5) A bucket and a counterpoise, each of mass M , are suspended by a fine string passing over a pulley, and a continuous shower of small inelastic particles falls into the bucket with a common velocity V , so that successive impacts take place at equal intervals of time. If m be the mass of the particles in the bucket at time t , show that the velocity of the bucket and the counterpoise is

$$m(V + \frac{1}{2}gt)/(2M + m).$$

With regard to Question (2): The tension required is that obtained by ignoring the motion of the pulley—the answer is given as $42\frac{2}{7}$ grams weight. If one assumes that the equivalent mass of the pulley is 40 grms. and that friction may be neglected, the question of finding the tensions in the two parts of the string is not much more difficult than the original question. They are $43\cdot2$ grms. wt. and $41\cdot6$ grms. wt.

Perhaps a better type of question would be the following—the experimental results are taken from Edser's *Physics*, page 15, but other results can easily be obtained from actual experiments:

"The masses in an Atwood's machine experiment were 135 and 125 grms. It was found that the acceleration of the system was 27 cms. per sec per sec. Find the tensions in the two parts of the string." [*Ans.* 131 grms. wt. and 128 grms. wt. approx.]

The other questions quoted, (1) and (3)-(5), are as far removed as possible from the type of problem which Atwood sought to illustrate by his machine. They seem to me, and I feel sure they seem to the average boy, mere conundrums made up to find out whether, from a mass of so-called "laws," he can select the particular rule which applies at each stage of the problem.

F. G. HALL.

129. I went to sit with the Commissioners at the Tower, where our Commission being read, we made some progress in business, our Secretary being Sir Geo. Wharton, that famous mathematician who writ the yearly Almanac during his Majesty's troubles. . . .—[The Salpêtre Commission] *Evelyn's Diary*, July 3, 1666.

130. Miss Hawkins in her *Memoirs* gives a story of Bennet Langton. He had been left one thousand pounds equally between himself and a man to whom he owed a hundred pounds. But when the deduction of the debt was made, to which he had agreed, he protested. When he was "shown by figures" that it was correct, he admitted that it might be true on paper, but claimed that it could not be true in practice.

A CENTURY AGO.

(v. *Math. Gazette*, vol ix. p. 169.)

THE following notes may help to explain a few references to names, etc., which perhaps are no longer familiar to the ordinary reader :

(1) Nos. 1 and 14 are as interesting to the undergraduate of 1916 as to his predecessors of 100 years ago.

(2) The paper, judging from No. 6 and from the rap in No. 24, is written by a Trinity man. One naturally thinks at first of Whewell, who took all knowledge for his province, and of his school-fellow Richard Owen. But the future Master was only just released from temporary bondage to the Queen of Sciences, being Second Wrangler and Second Smith's Prizeman in this year, and the great anatomist of a later day was at this time but twelve years old. So we must look elsewhere for the impulse which made the name of craniology familiar to the undergraduate world of 1816. Blumenbach's laborious description of his collection of skulls began in 1790 and ended in 1820. The founder of anthropology was the first to make popular the study of craniology, and about this time the "grand tour" was hardly complete without a visit to the Blumenbachian Museum on the part of those who made their leisurely way to the "University of Gottingen." Camper's famous "facial angle," and all that it implied, was accepted in France and America, but met with stormy criticism in Germany and Britain. Cogan translated in 1821 the memoirs of this artist-anthropologist, the first of which had been composed half a century before. Spix, Herder, and Doornik were, all three, suggesting various linear systems of measurement to express the relations of the several parts of the skull. Echoes of these controversies had no doubt reached Cambridge about this time, and interest may have been aroused in the subject of anthropology in general by the publication just three years before of Prichard's *Researches as to the Physical History of Man*. I do not know of any direct influence played by the University in the great work of systematisation. The only official who was bound to take a professional interest in the matter would be the Professor of Anatomy, whose Chair nine years before had celebrated its centenary, and the band of Professors of Divinity and the like who looked askance at all such subjects of investigation.

(3) No. 3. What was a cub? The reference to the "chace" suggests that this is a mere matter of cub-hunting.

(4) No. 6. Mr. Bradshaw could have told us in a moment what circumstances led to this public allusion to regrettable incidents in the history of the great institution over which he presided. As for the "fountain of the Nile," it is possible that news of Burckhardt's travels, 1812-1814, had come through by this time.

(5) No. 7. Discloses the fact that a wholesome suspicion had already arisen as to the universal applicability of what we now call the "military method" to the solution of problems.

(6) No. 8. May refer to the suffering inflicted by members of the school founded by Charles Simeon, at this time incumbent of Holy Trinity, Cambridge.

(7) No. 12. From the tone of political thought reflected in this question and in No. 10, this probably refers to Stanhope *quid* Chairman of the "Revolution Society"; as the winner of the self-explanatory ekename of "minority of one" in the Lords; as the husband of Pitt's sister, and the father of Lady Hester Lucy, the domineering damsel who bore with her very trying parent till she was twenty-four, when she fled from his roof and became the devoted companion of her uncle William Pitt; or as the subject of innumerable

caricatures from the pen of Gillray and his contemporaries. He died in the year this paper was published, thus giving us a limit for the month in which it appeared. For Flood, v. *D.N.B.* xix. 330. Sir Frederick Flood proposed that every anonymous book should have its author's name upon the title-page. The comet of 1811 was the celestial visitor which ought to have warned Napoleon of the waning of the influence of the great comet of 1769, which he had hitherto considered his own particular protecting genius. It heralded one of the great vintage years of history. It completed the suspicions of Sir W. Herschel as to the luminosity of comets in general. I am not aware that its eccentricity gave any trouble to Argelander or the other astronomers who investigated its orbit. If it is any comfort to know what its eccentricity was—it was 0.9951.

(8) No. 13. In these days of Bridge, one will have to remind readers that the card game *par excellence* is Whist, and that Hoyle's "Laws" held good in the final court of appeal till 1864. But Piquet must not be left in the lurch. It also has its points.

(9) No. 15. Was this the first appearance of the "lunar caustic" joke? Tschirnhausen gave the name of "caustic" in 1682 to the envelope of certain rays. For the epithet "lunar" I suspect we must go to astrology. De Morgan and Babbage both tell how the *Morning Post* of May 4, 1831, was gulled by a communication anent the claims of Mr. Goulburn to represent his University against Mr. Cavendish. The scientific attainments of Goulburn were described as by no means insignificant. Had he not succeeded in the exact rectification of the circular arc, and in discovering the equation of the lunar caustic, "a problem likely to prove of great value in nautical astronomy"? As De Morgan reminds us, the squib had in it poetic justice, for Goulburn was the minister who told the Astronomical Society deputation that the Government "did not care twopence for all the science in the country." Cavendish, on the other hand, had been Second Wrangler, and by the unanimous decision of Babbage, Airy, and Lax, was awarded the first Smith's Prize. De Morgan at first suspected his friend Sheepshanks of perpetrating the squib, especially as Gunning, the famous Esquire Bedell, of "Reminiscences" fame, according to the two Coopers (of the *Athenae Cantabrigienses*) attributed the authorship of the hoax to the future astronomer. But a long-continued intimacy with Sheepshanks convinced De Morgan that, though his friend was quite capable of such a jest, he had nothing to do with it. So he transferred his suspicions to Babbage. He says that Babbage "gives it in his Passages, etc., and is evidently writing from memory, for he gives the wrong year. But he has given the paragraph, though not accurately, yet with such a recollection of the points as brings suspicion of the authorship upon him, perhaps in conjunction with D. B." [Drinkwater Bethune].

(10) No. 20. The last twelve of the Junior optimes used to be called the twelve apostles, which may possibly explain the reference to the Holy Land. "Wooden Spoon" is for ever embalmed in "Don Juan" as: "the name with which we Cantabs please To dub the last of honours in degrees." But I am floored by "Clapham Common," which has a Georgian-"Quarterly" flavour about it; unless it came into existence to indicate the probable level of mathematical culture to be expected from the neighbourhood associated with the activities of the Clapham Sect, identified with the Stephens family, and the names of Thornton, Zachary Macaulay, and others. That this may be the case is not unlikely, for Hood selected, for his parody on Gray's Ode, "A Clapham Academy" as the most obvious antithesis to the "Eton" of his day.

(11) No. 21. If this puzzles any one, let him read "hyperbole."

(12) No. 23. Wood's *Algebra* was a well-known text-book at Cambridge. The curious in such matters may be interested in knowing that nearly 30,000 copies were sold in half a century. For the first five editions, 1795,

1798, 1801, 1806, and 1810, the sales were 1000, 1000, 1500, 2000, and 2500 respectively; and were then 3000 for each of the editions of 1815 onwards, the editions appearing at intervals of five years, with the exception of that of 1840, which, with Mr. Lund's additions, came out in 1841. The cause of the delay of a year was not the additions, for they were appended to the last 400 copies of the edition of 1835. James Wood was Senior Wrangler in 1782, and became Master of St. John's (1815-1839).

(13) No. 24. Animal nicknames were at one time allotted to most of the Colleges. For some reason or other the Johnian "pig" has survived, perhaps aided by the "crackling," i.e. the three velvet bars on the arms of the Johnian gown. For the story of the origin of "Johnian hogs," as told in the early days of the last century, v. the note to Christopher Smart's song "The Pretty Bar-Keeper of the Mitre," in *The Cambridge Tart* (1823. Stanza vii. p. 41; note, p. 279). The "Bridge of Sighs" perhaps helped to perpetuate the name, being conveniently like the "Bridge or Isthmus of Suez." It is necessary, alas! in these days to remind most young mathematicians that Suez somewhat resembles in sound the Greek for "pigs."

W. J. G.

131. He [the tailor] first took my altitude by a quadrant, and then, with rule and compasses, described the dimensions and outlines of my whole body, all which he entered on paper.—*Gulliver's Travels*, c. ii. *Laputa*.

132. For any skill in geometry, I dare not commend him; for he never yet could find out the dimensions of his own conscience.—*Overbury's Characters*. *A Tylour*.

133. I dined at the Earl of Sunderland's with Lord Spencer. My Lord shew'd me his Library, now again improv'd by many books bought at the sale of Sir Charles Scarborough, an eminent physician, which was the very best collection, especially of mathematical books, that was I believe in Europe, once designed for the King's Library at St. James's, but the Queene's dying, who was the great patronesse of that designe, it was let fall, and the books were miserably dissipated.—*Evelyn's Diary*, March 10, 1695.

134. Dr. Kay, Principal of Bishop's College, Calcutta, heard E. B. Cowell, Professor of History at Presidency College, say: "If anything were wanted to prove the existence of an evil Providence such as that we attribute to the Arch Enemy, it would be enough to refer to the preparation of men's minds for Colenso's heresies by the wide diffusion of his manuals of Arithmetic and kindred subjects."—*Life and Letters of Edward Byles Cowell*, 1904, p. 425.

[Whately wrote a pamphlet entitled *Historic Doubts on the Existence of Napoleon Buonaparte*. Can any reader remember a similar *jeu d'esprit* written to prove that Colenso did not write the heretical books attributed to him?]

135. Is Algebra rightly termed analysis? Edgar A. Poe, a very queer American author, maintains the negative: he also enters into the question whether games of skill and chance are useful to the analytical powers, and gives the preference to draughts over chess, and to whist over either. But he seems to think that the chief applications of analysis are to the interpretations of cryptographies, the disentanglement of police puzzles, and the solution of charades.

There is, however, plausibility in his theory that a good analyst must be both poet and mathematician. This is Ruskin's "imagination penetrative"; such a faculty belongs to the minds of Verulam, Newton, Kepler and Galileo. I do not, however, see the necessity of Ruskin's threefold division of the imaginative faculty. Would not imagination analytic and creative suffice?... —Mortimer Collins.

[It may be remembered that John Wallis was employed by the Parliamentarians from 1642 to 1645 to decipher intercepted royalist messages.]

AN ANALYTICAL REMAINDER FORMULA.

BY PROF. D. M. Y. SOMMERVILLE, D.Sc.

It is proposed to find an expression, in terms of periodic functions, for the least positive residue $R(n/p)$ when n is divided by p , n and p being positive integers.

Let ω be a primitive p th root of unity, so that

$$\begin{aligned}\omega &= \cos 2\pi/p + i \sin 2\pi/p, \\ \omega^p &= 1, \\ \omega^{p-1} + \omega^{p-2} + \dots + 1 &= 0.\end{aligned}$$

Then consider the function

$$f(n) \equiv \omega^{(p-1)n} + \omega^{(p-2)n} + \dots + \omega^n + 1.$$

If $n \equiv 0 \pmod{p}$, $f(n) = p$. If $n \not\equiv 0 \pmod{p}$, suppose first that n is prime to p . Then the residues, mod p , of the indices of ω are all incongruent, and

$$f(n) \equiv \omega^{p-1} + \omega^{p-2} + \dots + \omega + 1 = 0.$$

Next suppose that n and p have a highest common factor g , so that $p = gq$. Then the residues are only $0, g, 2g, \dots, (q-1)g$. Let $\omega^g = w$; then w is a primitive q th root of unity, and

$$f(n) = g(w^{q-1} + w^{q-2} + \dots + w + 1) = 0.$$

Hence in all cases $f(n) = p$ if $n \equiv 0 \pmod{p}$, and vanishes for all other integral values of n .

Similarly, $f(n-r) = p$ if $n \equiv r \pmod{p}$, and vanishes for all other integral values of n, r being an integer.

We can now construct an expression for $R(n/p)$, viz.

$$R(n/p) = \frac{1}{p} \sum_{r=1}^{p-1} r f(n-r). \dots\dots\dots (1)$$

We shall next obtain an expression in the form of a finite Fourier series. Let $F(r)$ be the real part of $f(r)$, so that for all integral values of r ,

$$f(r) = F(r) = \sum_{k=1}^{p-1} \cos 2\pi kr/p + 1;$$

$$\text{then } R(n/p) = \frac{1}{2}(p-1) + \frac{1}{p} \sum_{k=1}^{p-1} \sum_{r=1}^{p-1} r \cos \frac{2\pi k(n-r)}{p}. \dots\dots\dots (2)$$

Expanding the cosines, the coefficient of $\cos 2\pi kn/p$ is

$$C = \frac{1}{p} \sum_{r=1}^{p-1} r \cos \frac{2\pi kr}{p}.$$

$$\begin{aligned}\text{Now } \cos 2\pi k(p-r)/p &= \cos 2\pi kr/p; \text{ hence} \\ 2C &= \sum_{r=1}^{p-1} \cos \frac{2\pi kr}{p} = -1.\end{aligned}$$

Hence the part due to the cosines is

$$-\frac{1}{2} \sum_{k=1}^{p-1} \cos \frac{2\pi kn}{p}.$$

If p is odd, this can be written

$$-\frac{1}{2} \sum_{k=1}^{(p-1)} \cos \frac{2\pi kn}{p};$$

and if p is even,

$$-\frac{1}{2} \sum_{k=1}^{(p-2)} \cos \frac{2\pi kn}{p} - \frac{1}{2} \cos \pi n.$$

The coefficient of $\sin 2\pi kn/p$ is

$$S = \frac{1}{p} \sum_{r=1}^{p-1} r \sin \frac{2\pi kr}{p}.$$

If p is odd, this can be written

$$-\frac{1}{p} \sum_{r=1}^{\frac{1}{2}(p-1)} (p-2r) \sin \frac{2\pi kr}{p};$$

and if p is even,

$$-\frac{1}{p} \sum_{r=1}^{\frac{1}{2}(p-2)} (p-2r) \sin \frac{2\pi kr}{p}.$$

Also the coefficient of $\sin 2\pi(p-k)n/p$, which $= -\sin 2\pi kn/p$, is

$$\frac{1}{p} \sum_{r=1}^{p-1} r \sin \frac{2\pi(p-k)r}{p} = -\frac{1}{p} \sum_{r=1}^{p-1} r \sin \frac{2\pi kr}{p}.$$

Hence the part due to the sines is, if p is odd,

$$-\frac{2}{p} \sum_{k=1}^{\frac{1}{2}(p-1)} \sum_{r=1}^{\frac{1}{2}(p-1)} (p-2r) \sin \frac{2\pi kr}{p} \sin \frac{2\pi kn}{p};$$

and if p is even,

$$-\frac{2}{p} \sum_{k=1}^{\frac{1}{2}(p-2)} \sum_{r=1}^{\frac{1}{2}(p-2)} (p-2r) \sin \frac{2\pi kr}{p} \sin \frac{2\pi kn}{p}.$$

Hence, if p is odd,

$$R(n/p) = \frac{1}{2}(p-1) - \sum_{k=1}^{\frac{1}{2}(p-1)} \cos \frac{2\pi kn}{p} - \frac{2}{p} \sum_{k=1}^{\frac{1}{2}(p-1)} \sum_{r=1}^{\frac{1}{2}(p-1)} (p-2r) \sin \frac{2\pi kr}{p} \sin \frac{2\pi kn}{p};$$

if p is even,

$$R(n/p) = \frac{1}{2}(p-1 - \cos \pi n) - \sum_{k=1}^{\frac{1}{2}(p-2)} \cos \frac{2\pi kn}{p} - \frac{2}{p} \sum_{k=1}^{\frac{1}{2}(p-2)} \sum_{r=1}^{\frac{1}{2}(p-2)} (p-2r) \sin \frac{2\pi kr}{p} \sin \frac{2\pi kn}{p}.$$

The same method can be applied in other similar problems involving periodicity. To take a concrete one: *find an expression for the number of ship's bells corresponding to any hour or half hour.* Neglecting dog-watches, if h is the number of the hour and B the number of bells, the following are corresponding values:

$$h = 4n, 4n + \frac{1}{2}, 4n + 1, 4n + \frac{3}{2}, \text{ etc.},$$

$$B = 8, 1, 2, 3, \text{ etc.},$$

where $n = 1, 2$ or 3 .

We then form the function $f(r) = 1 + \omega^r + \omega^{2r} + \dots + \omega^{(p-1)r}$, where ω is a primitive p th root of unity, and denote by $F(r)$ its real part. Then

$$B = F(2h) + \frac{1}{p} \sum_{r=1}^{p-1} r F(2h-r),$$

which on reduction becomes

$$B = 4\frac{1}{2} + \cos 90h + \cos 180h + \cos 270h + \frac{1}{2} \cos 360h \\ - (\sqrt{2} + 1) \sin 90h - \sin 180h - (\sqrt{2} - 1) \sin 270h,$$

the angles being now in degrees.

The importance of this formula for a ship's officer can scarcely be underestimated.

D. M. Y. SOMMERVILLE.

Victoria Univ. Coll., Wellington, N.Z., Feb. 1922.

ITERATIVE PROCESSES.*

By H. TODD, B.A.

I. If $(1 + \sqrt{2})^n = p + q\sqrt{2}$, where p and q are positive integers, then $(1 - \sqrt{2})^n = p - q\sqrt{2}$, and by multiplication,

$$p^2 - 2q^2 = (1 + \sqrt{2})^n (1 - \sqrt{2})^n = (-1)^n.$$

$$\therefore p - q\sqrt{2} = (-1)^n / (p + q\sqrt{2}) = (-1)^n / (2q\sqrt{2}) \text{ very nearly,}$$

since $|p - q\sqrt{2}|$ is small.

Hence $p/q - \sqrt{2} = (-1)^n / (2q^2\sqrt{2})$, so that p/q furnishes a good approximation to $\sqrt{2}$, in fact the successive results for $n=1, 2, \dots$ are $1/1, 3/2, 7/5, 17/12, 41/29, 99/70, \dots$, of which the last is in error by less than 1 part in 13,800. This is a well-known result and suggests similar possibilities for \sqrt{n} .

Let a and b be any real numbers not involving \sqrt{n} .

$$\text{Then} \quad (a + b\sqrt{n})^r = p + q\sqrt{n},$$

$$\text{and} \quad (a - b\sqrt{n})^r = p - q\sqrt{n}.$$

Solving these equations for p and q , we find that

$$\begin{aligned} p/(q\sqrt{n}) &= [(a + b\sqrt{n})^r + (a - b\sqrt{n})^r] / [(a + b\sqrt{n})^r - (a - b\sqrt{n})^r] \\ &= [1 + (a - b\sqrt{n})^r / (a + b\sqrt{n})^r] / [1 - (a - b\sqrt{n})^r / (a + b\sqrt{n})^r]. \end{aligned}$$

Now the absolute value of $(a - b\sqrt{n}) / (a + b\sqrt{n})$ is less than unity; therefore $\lim_{r \rightarrow \infty} p/(q\sqrt{n}) = 1$, i.e. p/q tends to the limit \sqrt{n} .

Hence to find \sqrt{n} it is only necessary to raise $(a + b\sqrt{n})$ to any power and take the ratio of the coefficients of 1 and \sqrt{n} , and it may be noticed here that, (i) a and b should be chosen initially to make $|a - b\sqrt{n}|$ small in order that rapid convergence may ensue, and (ii) if errors are made in the calculation they will ultimately disappear, for an error would merely mean a change in a and b from which the approximation was started.

II. For cube roots a similar process holds good; that is, if $x = \sqrt[3]{n}$ and a, b, c are any real positive numbers not involving $\sqrt[3]{n}$; then, if

$$(a + bx + cx^2)^r = p + qx + sx^2,$$

we shall find that p/q and q/s both tend to $\sqrt[3]{n}$ as $r \rightarrow \infty$.

To prove this statement, let ω be a complex cube root of unity

$$(\omega = \frac{1}{2}(-1 + \sqrt{-3}));$$

then ω^2 is another.

Also put

$$\lambda_1 = a + bx + cx^2,$$

$$\lambda_2 = a + b\omega x + c\omega^2 x^2,$$

$$\lambda_3 = a + b\omega^2 x + c\omega x^2.$$

Then

$$\lambda_1^r = p + qx + sx^2,$$

$$\lambda_2^r = p + q\omega x + s\omega^2 x^2,$$

$$\lambda_3^r = p + q\omega^2 x + s\omega x^2.$$

Solving these equations for p, q, s , and recalling that $1 + \omega + \omega^2 = 0$, we find that

$$\begin{aligned} p/(qx) &= [\lambda_1^r + \lambda_2^r + \lambda_3^r] / [\lambda_1^r + \omega^2 \lambda_2^r + \omega \lambda_3^r] \\ &= \left[1 + \left(\frac{\lambda_2}{\lambda_1} \right)^r + \left(\frac{\lambda_3}{\lambda_1} \right)^r \right] / \left[1 + \omega^2 \left(\frac{\lambda_2}{\lambda_1} \right)^r + \omega \left(\frac{\lambda_3}{\lambda_1} \right)^r \right]. \end{aligned}$$

* A paper read to the Bristol branch of the Mathematical Association.

But

$$\begin{aligned}
 |\lambda_2| &= |a + b\omega x + c\omega^2 x^2| \\
 &< |a| + |b\omega x| + |c\omega^2 x^2|, \\
 \text{i.e.} \quad &< a + b|x| + c|x|^2 \quad (\text{since } |\omega| = |\omega^2| = 1), \\
 \text{i.e.} \quad &< \lambda_1.
 \end{aligned}$$

Similarly $|\lambda_3| < \lambda_1$, so that both $(\lambda_2/\lambda_1)^r$ and $(\lambda_3/\lambda_1)^r$ tend to zero as $r \rightarrow \infty$.

Hence $p/(qx) \rightarrow 1$, or $p/q \rightarrow x$, and similarly it may be shown that $q/s \rightarrow x$.

Once again it is evident that if a, b, c are suitably chosen, the convergence can be made very rapid; for example, if $x = \sqrt[3]{5}$, it will be found that $a=41$, $b=24$, and $c=14$ gives for the second power approximations valid to four places. Also it is evident that any errors committed in the work will ultimately disappear for the same reason as before.

III. For the n^{th} roots we shall merely state the theorem as follows, the proof being an obvious extension of that for the previous case.

If $x = \sqrt[n]{r}$, and

$$(a + bx + cx^2 + \dots + kx^{n-1})^p = A + Bx + Cx^2 + \dots + Kx^{n-1},$$

where a, b, c, \dots, k are real positive quantities not involving $\sqrt[n]{r}$; then in the limit, as $p \rightarrow \infty$, the ratios $A/B, B/C, C/D, \dots$ all tend to $\sqrt[n]{r}$.

As an example take $x = \sqrt[4]{2}$, and $a=b=c=d=1$.

$$\text{Now} \quad (1 + x + x^2 + x^3)^4 = 195 + 164x + 138x^2 + 116x^3,$$

and $195/164 = 1.18902\dots$, the correct result being $1.1893\dots$ (For the actual multiplication a method of detached coefficients shortens the work, remembering that $x^4 = 2x$, etc.)

IV. These results in the solution of the equation $x^n = r$ suggest that similar results will hold for the roots of any algebraic equation, and with certain restrictions this proves to be the case. Let $x^2 + px - q = 0$ be any quadratic whose roots are real and different, being x_1 and x_2 , with $|x_1| > |x_2|$.

Then x^n will reduce to a linear function of x in virtue of $x^2 = -px + q$.

$$\text{Let} \quad x^n = a_n x + b_n.$$

$$\text{Then} \quad x_1^n = a_n x_1 + b_n,$$

$$\text{and} \quad x_2^n = a_n x_2 + b_n.$$

So, by solving these equations for a_n and b_n ,

$$\begin{aligned}
 a_n/b_n &= (x_1^n - x_2^n)/(x_1 x_2^n - x_2 x_1^n) \\
 &= [1 - (x_2/x_1)^n]/[1 - x_2/x_1 (x_2/x_1)^n].
 \end{aligned}$$

But $|x_1| > |x_2|$, so that $(x_2/x_1)^n \rightarrow 0$ as $n \rightarrow \infty$.

$$\text{Hence} \quad a_n/b_n \rightarrow -1/x_2 = x_1/q.$$

So this gives a method of finding the roots of a quadratic when they are real.

$$\text{Example.} \quad x^2 = x + 1, \quad \text{here, } x_1 = \frac{1}{2}(\sqrt{5} + 1) = 1.618\dots,$$

$$x^3 = 2x + 1, \quad q = 1,$$

$$x^6 = 8x + 5, \quad 144/89 = 1.6179\dots$$

$$x^{12} = 144x + 89;$$

It is easily proved that the magnitude of the error committed in taking a_n/b_n for x_1/q is of the order of $(x_2/x_1)^{n-1} \cdot x_1/q$, which furnishes a means of knowing how far to proceed in order to get an approximation to any desired degree of accuracy.

V. The extension to cubics is as follows:

If $x^3 + px^2 + qx + r = 0$ is an equation whose root x having the greatest modulus is real, and if x^n be reduced in virtue of $x^3 = -px^2 - qx - r$ to the form

$$x^n = \lambda_n + \mu_n x + \nu_n x^2, \dots \dots \dots (1)$$

we shall have in the limit,

$$\lambda_n/(x^3 + px + q) = \mu_n/(x + p) = \nu_n/1.$$

The proof of this statement is similar to the quadratic case, for if x_1, x_2 are the other roots of the original cubic, we get more equations from (i) with x_1, x_2 respectively substituted for x . These are solved for λ_n, μ_n, ν_n , and the limit is found as before.

It is not difficult to prove that the error committed in taking μ_n/ν_n for $x+p$ is of order $A(x_1/x)^n + B(x_2/x)^n$, where A and B are independent of n , and if x_1 and x_2 are complex numbers, the error is of order $C\sqrt{x^{-2n}}$, where C is independent of n .

The case of chief interest is Cardan's "irreducible case," for then all the roots are real and the conditions are satisfied.

Example. $x^3 = x + 1$: in this case the other roots x_1 and x_2 are complex, but

$$|x_1| = |x_2| < x;$$

$$x^9 = 3x^2 + 4x + 2;$$

$$x^{18} = 37x^2 + 49x + 28.$$

$49/37 = 1.32\dots$, and the required result is $1.3247\dots$.

VI. All the foregoing results are special cases of a general theorem concerning algebraic equations (and itself a special case of a theorem on linear transformations) that it may be of interest to state.

Let $x^n + p_1x^{n-1} + p_2x^{n-2} + \dots + p_n = 0$ be any algebraic equation with real coefficients whose roots are $x, x_1, x_2, \dots, x_{n-1}$.

Let $\varphi(x) = a + bx + cx^2 + \dots + kx^{n-1}$, where a, b, \dots, k are real, and

$$[\varphi(x)]^r = a_r + b_rx + c_rx^2 + \dots + j_rx^{n-2} + k_rx^{n-1}.$$

Then if x is real and $|\varphi(x)| > |\varphi(x_1)|, |\varphi(x_2)|, \dots, |\varphi(x_{n-1})|$, we have in the limit as $r \rightarrow \infty$,

$$a_r/(x^{n-1} + p_1x^{n-2} + \dots + p_{n-1}) = b_r/(x^{n-2} + p_1x^{n-3} + \dots + p_{n-2}) = \dots = j_r/(x + p_1) = k_r/1.$$

H. TODD.

136. Of all the pursuits of human ingenuity, that of mathematics demands the intensest application. It is related of one well known in the records of science, that after the exhaustion of some minute astronomical experiments he has been driven to count the drops of rain at the window, or watch the race of two flies along the glass, in order that by an utter repose of thought the intellect might recover its elasticity.—*Conversations at Cambridge, 1836.* [Who was the astronomer?]

137. "... Given certain factors, and a sound brain should always evolve the same fixed product with the certainty of Babbage's calculating machine.

"What a satire, by the way, is that machine on the mere mathematician! A Frankenstein-monster, a thing without brains and without heart, too stupid to make a blunder; that turns out results like a corn-sheller, and never grows any wiser or better, though it grind a thousand bushels of them!

"I have an immense respect for a man of talents *plus* 'the mathematics.' But the calculating power alone should seem to be the least human of qualities, and to have the smallest amount of reason in it; since a machine can be made to do the work of three or four calculators, and better than any one of them. Sometimes I have been troubled that I had not a deeper intuitive apprehension of the relations of numbers. But the triumph of the ciphering hand-organ has consoled me. I alway fancy I can hear the wheels clicking in a calculator's brain. The power of dealing with numbers is a kind of 'detached lever' arrangement, which may be put into a mighty poor watch. I suppose it is about as common as the power of moving the ears voluntarily, which is a moderately rare endowment."—Oliver Wendell Holmes, *The Autocrat of the Breakfast-Table*, 1858, chap. i. [Per Dr. J. M'Whan.]

REVIEWS.

A Treatise on Probability. By JOHN MAYNARD KEYNES. Pp. xi+466. 18s. net. 1921. (Macmillan.)

This is undoubtedly the most important work on probability that has appeared for a very long time. Its importance is especially as regards the logical foundations of the subject, which are usually treated with extraordinary carelessness. I propose first to give an abstract of Mr. Keynes's book, then to discuss briefly some of its more disputable portions.

Part I. deals with "Fundamental Ideas," and Chapter I. deals with "The Meaning of Probability." Mr. Keynes holds that a formal definition of probability is impossible, since he regards it as part of the fundamental apparatus of logic. Logic is accustomed to considering that relation between premiss and conclusion which enables us to infer the latter with certainty from the former, but this Mr. Keynes regards as only the extreme degree of the probability-relation, which subsists whenever one proposition has any bearing, favourable or unfavourable, upon the truth of another. Thus a proposition does not have a probability in itself, but only in relation to certain data. It may have different probability-relations to different sets of data, and all these are equally justified. Of course when we want to use probability as a "guide to life," in Bishop Butler's phrase, we must take account of all relevant knowledge; but our estimate of probability relative to *that* knowledge will not be falsified by subsequent knowledge leading to a different estimate.

The probability of a proposition *a* relative to data *h* is represented by a/h . This is the fundamental symbol of the book. The inclusion of *h* in this symbol represents and entails much of Mr. Keynes's philosophy. It is remarkable how often the logic and philosophy of mathematical concepts has gone astray through the employment of symbols which did not contain explicitly all the variables upon which their value depended. If one were to employ such a symbol as (say) " $P(a)$ " for "the probability of *a*," adding a proviso that this should be taken relatively to certain data *h*, it would be impossible practically to remember or express the relevance of *h*, and one's analysis would be certain to suffer sooner or later. The introduction of the symbol a/h is therefore of great importance. This symbol means the degree to which *h* makes *a* probable, or, as it may be expressed, the degree of belief which it is rational to give to *a* when *h* constitutes our relevant knowledge.

Mr. Keynes distinguishes between direct and indirect knowledge or rational belief, the latter being that part which rests upon argument. When the probability-relation between *a* and *h* is certainty ($a/h=1$), we can *know a* when we know *h*; this is indirect knowledge of *a*. But when the probability-relation is other than certainty, *h* does not enable us to know *a*, but only to attach a certain degree of rational belief to it. Probability, unlike certainty, does not enable us to *know* any proposition which omits mention of the premiss.

Mr. Keynes maintains that probabilities are not always, even in theory, capable of numerical measurement. He maintains that it is not the case that of two unequal probabilities one must be the greater. To use an illustration (not Mr. Keynes's), the order of probabilities is like that of points on meridians of longitude: all lie between the North Pole (certainty) and the South Pole (impossibility), but points on different meridians do not lie on the same series. One of the meridians (that of Greenwich no doubt) represents numerically measurable probabilities; all the others represent probabilities which have no numerical measure. Mr. Keynes compares probabilities with similarities: there are degrees of similarity, but we cannot say that one similarity is always greater than, less than, or equal to, another. "For instance, a book bound in blue morocco is more like a book bound in red morocco than if it were bound in blue calf; and a book bound in red calf is more like the book in red morocco than if it were in blue calf. But there may be no comparison between the degree of similarity which exists between books bound in red morocco and blue morocco, and that which exists between books bound in red morocco and red calf." The chapter which discusses this subject abounds in good illustra-

tions. For example, there is the lady to whom a jury awarded £100 for having been baulked of her chance of winning a prize in a beauty competition. Mr. Keynes holds that she got too much; evidently he thinks it *probable*, on the data, that she will not read his book.

The cases in which numerical measurement of probabilities is possible arise where there are a set of exclusive and exhaustive alternatives which are all equally probable on the data. This requires some principle by which we can judge directly, in suitable cases, that two probabilities are equal. The accepted principle is that of *non-sufficient reason*, or, as Mr. Keynes prefers to call it, of *indifference*. In its traditional form, this principle "asserts that if there is no *known* reason for predicating of our subject one rather than another of several alternatives, then relatively to such knowledge the assertions of each of these alternatives have an *equal* probability." This principle obviously requires limitation. *E.g.* if *A* is an object about which we have no information bearing on the question whether it is red or whether it is a book, the principle allows us to argue that the probability of "*A* is red" is $\frac{1}{2}$, and so is the probability of "*A* is a red book," whence it follows that if *A* is red it is a book. Mr. Keynes concludes that the principle in the above form is a necessary but not a sufficient condition of equiprobability. In order to arrive at a sufficient condition, it is first necessary to define *irrelevance*. It would be natural to say: h_1 is irrelevant to x on evidence h if $x/hh_1 = x/h$, i.e. if the addition of h_1 to our data makes no difference to the probability of x . But h_1 may consist of two parts, one of which increases the probability of x while the other diminishes it. To exclude this possibility, we define: h_1 is *irrelevant* to x/h if there is no proposition h_1' , inferrible from h_1h but not from h , such that $x/h_1'h \neq x/h$. A proposition is defined as *relevant* when it is not irrelevant.*

By means of this definition, Mr. Keynes arrives at a more satisfactory enunciation of the principle of indifference: "There must be no *relevant* evidence relating to one alternative, unless there is *corresponding* evidence relating to the other; our relevant evidence, that is to say, must be symmetrical with regard to the alternatives, and must be applicable to each in the same manner." When this condition is fulfilled, the alternatives are equally probable on the data. The discussion affects one of the stock elementary problems of probability. Given an urn containing white and black balls in unknown proportions, are we to suppose each ratio of white to black equally probable *a priori*, or are we to suppose that each ball is equally likely *a priori* to be white or black? The former is the commoner view, but the latter is alone correct.

There is need, however, of one further condition before the principle of indifference becomes applicable. The alternatives must be "indivisible," i.e. neither must be "capable of being further split up into a pair of possible but incompatible alternatives of the same form as the original pair." (For the formal definition, see p. 60.)

The principle of indifference allows, where it is applicable, numerical measurement of probabilities; for if there are n equally probable alternatives which are exhaustive and exclusive, the probability of each is $\frac{1}{n}$. But comparisons of greater and less are possible in many cases where numerical measurement is impossible. We can compare the probabilities ab/h and a/h , and also the probabilities a/hh_1 and a/h . In the first case we keep the data constant and increase the conclusion; in the second, we keep the conclusion constant and increase the data. By means of these, many other comparisons become indirectly possible.

Part I. concludes with two chapters, the first historical, the second dealing with the Frequency Theory advocated by Venn and many other writers. I shall return to this theory when I come to discuss Mr. Keynes's fundamental theses.

* I do not know whether Mr. Keynes has considered and rejected a definition of irrelevance which, *prima facie*, would be simpler than his. He does not state definitely whether *every* pair of propositions has some probability-relation, but I think he does not hold this view. I think he would say, e.g., that there is no probability-relation between the propositions ' $2+2=4$ ' and 'Napoleon disliked poodles.' If so, it would seem natural to define h as irrelevant to a when a/h does not exist.

Part II., "Fundamental Theorems," gives the definitions and axioms upon which the formal reasoning of the rest of the book is based, together with some propositions readily derivable from the definitions and axioms. It is impossible to summarize this Part, since it is already as condensed as possible. I cite only, as essential to the whole formal structure, the definitions of addition and multiplication :

Addition : $ab/h + a\bar{b}/h = a/h$ (where \bar{b} stands for not $-b$).

Multiplication : $ab/h = a/bh, b/h = b/ah, a/h$.

Thus addition and multiplication are not defined for any pair of probabilities, but only for such as have certain forms. This, of course, is connected with Mr. Keynes's view that probabilities are in general non-numerical. It is surprising how successful he is in raising his mathematical superstructure upon foundations which might have been thought inadequate to support it.

Part III., "Induction and Analogy," is the most important in the book from the point of view of philosophy and theory of knowledge. It has long since been proclaimed from the house-tops that all scientific knowledge rests upon induction, and it has been fairly evident that conclusions reached by induction were not certain, but at best more or less *probable*. Hume rejected induction as merely a fallacious habit ; Mill tried to rehabilitate it. But it was soon seen that Mill's theories were mere eyewash, yet no better theories were invented to take their place. The philosophers had nothing sensible to offer to remedy the scandal. The writers on probability, on the other hand, offered too much : there was Laplace's theorem, according to which, when an event has happened m times, and failed n times, the chance that it will happen next time is $\frac{m+1}{m+n+2}$. This served admirably for proving that the sun will

probably rise to-morrow. But it also proved that, if you ask the first man you meet in a village whether his name is Ebenezer Stick-in-the-mud, it is even odds that he will reply in the affirmative ; and if he does, it is two to one that the next man you meet will have the same name. Mr. Keynes makes short work of this theorem, which depends (among other errors) upon a wrong formulation of the principle of indifference. What he contributes is (as he admits) not yet a full and complete logic of induction, but I think it must form a part of the complete theory, and I do not think one can say as much for anything previously written on the subject.

Mr. Keynes maintains that Analogy is more fundamental than induction from mere number of instances. One may say that the purpose of inductive arguments is to prove, by examination of instances, that two characteristics A and B , which are associated in those instances, are *probably* always associated. The object in practice is to make the probability as great as possible, and the object of theory is to show how to increase it and to discover circumstances (if there are such) under which it approaches certainty as a limit.

Given a set of instances all of which have the characteristics A and B , Mr. Keynes gives the name "positive analogy" to those characteristics which belong to all the instances, and the name "negative analogy" to those characteristics which belong to some but not all of the instances. None of these latter are invariably associated with B , therefore A 's chance of being invariably associated with B is improved whenever the negative analogy is increased, just as one's chance of winning in a lottery is improved whenever some other competitor is found to have not drawn a prize. This assumes that there are prizes, i.e. that there are correlations. Mr. Keynes considers some assumption of this kind essential to the validity of the inductive method ; this is a point to which we shall return later. He also holds that the only importance of numerous instances in induction lies in the probability that they may increase the negative analogy, since they may differ in some respect even if they are not known to do so. He gives the name "Pure Induction" to the argument from number of instances alone, where it is not *known* that they increase the negative analogy. The chief value of his discussion of Pure Induction, he says, "is negative, and consists in showing that a line of advance, which might have seemed promising, turns out to be a blind alley, and that we are thrown back on known Analogy." But his discussion is so interesting that it must be reproduced in outline.

Let h represent the *à priori* data, g the generalization we wish to establish; and $x_1, x_2, x_3, \dots x_n$ instances of g . Let p_n represent the probability of the generalization when $x_1, x_2, \dots x_n$ have been observed, *i.e.*

$$p_n = g/hx_1x_2 \dots x_n,$$

and let y_{n+1} represent the probability, in the same circumstances, that the next instance will verify the generalization, *i.e.*

$$y_{n+1} = x_{n+1}/hx_1x_2 \dots x_n.$$

Put $p_0 = g/h$ = the *à priori* probability of the generalization. Then we find

$$p_n = \frac{1}{y_1y_2 \dots y_n} \cdot p_0,$$

which leads to

$$p_n = \frac{p_0}{p_0 + x_1x_2 \dots x_n/\bar{g}h(1 - p_0)},$$

where \bar{g} stands for the falsehood of g . If we are to be able to have confidence in a pure induction when the instances are numerous, it is necessary that p_n should have 1 for its limit as n increases. This requires that $x_1x_2 \dots x_n/\bar{g}h \cdot \frac{1}{p_0}$ should approach 0 as a limit as n increases. This will be the case if, for a specified s , p , exceeds 0 by a finite amount (*i.e.* is equal to or greater than some finite numerical probability), and, whenever $r > s$, $x_r/x_1x_2 \dots x_{r-1}\bar{g}h$ falls short of certainty by a finite amount.

"In other words Pure Induction can be usefully employed to strengthen an argument if, after a certain number of instances have been examined, we have, from some other source, a finite probability in favour of the generalization, and, assuming the generalization is false, a finite uncertainty as to its conclusion being satisfied by the next hitherto unexamined instance which satisfies its premises."

It might be thought that these conditions could be often fulfilled. But Mr. Keynes argues (and I do not see how to refute him) that we can have no assurance that p_0 (or p_s) exceeds some finite numerical probability, unless we bring in some new principle to confer a finite degree of probability on untested generalizations. Some such principle must therefore be sought.

Traditionally, the law of causation and the law of the uniformity of nature were supposed to fulfil the required rôle. Mr. Keynes does not find much use for either of these. He interprets the law of the uniformity of nature as meaning that position in time and space are irrelevant to a generalization, and asks in a footnote: "Is this interpretation of the Principle of the Uniformity of Nature affected by the Doctrine of Relativity?" Although the point is not perhaps very important in relation to the theory of induction, it is perhaps worth while to observe that the principle is vitally affected by relativity, being reduced from a law of nature to a tautology, since any two distinct events *must* differ otherwise than in absolute position, if there is no such thing as absolute position. Mr. Keynes proceeds:

"The kind of fundamental assumption about the character of material laws, on which scientists appear commonly to act, seems to me much less simple than the bare principle of Uniformity. They appear to assume something much more like what mathematicians call the principle of the superposition of small effects, or, as I prefer to call it, in this connection, the *atomic* character of natural law. The system of the material universe must consist, if this kind of assumption is warranted, of bodies which we may term (without any implication as to their size being conveyed thereby) *legal atoms*, such that each of them exercises its own separate, independent, and invariable effect, a change of the total state being compounded of a number of separate changes each of which is solely due to a separate portion of the preceding state."

This assumption does not yet afford a justification of induction, but it is not very different from the kind of assumption which Mr. Keynes finds necessary. He introduces what he calls the "independent variety" of a system of facts or propositions, which consists of the logical minimum by means of which everything else in the system can be defined or demonstrated. When the number of premisses constituting the independent variety is finite, the system is defined as finite. (Thus *e.g.* pure mathematics is a finite system.) Mr.

Keynes proves that "if the premisses of our argument permit us to assume that the facts or propositions, with which the argument is concerned, belong to a *finite* system, then probable knowledge can be validly obtained by means of an inductive argument." He suggests that, in any scientific inquiry which uses inductive methods, our procedure is not logically justifiable unless we assume (or have some reason to suppose) that we are dealing with a finite system in the above sense. A finite probability that we have to do with such a system suffices. But unless we can believe that there is such a finite probability, Mr. Keynes offers us no justification for the habit of employing inductive arguments.

The question of induction and analogy is so important that it seems desirable to set forth Mr. Keynes's views in somewhat greater detail. First, he reduces induction to analogy; then he finds that

"As a logical foundation for Analogy, therefore, we seem to need some such assumption as that the amount of variety in the universe is limited in such a way that there is no one object so complex that its qualities fall into an infinite number of independent groups (*i.e.* groups which might exist independently as well as in conjunction); or rather that none of the objects about which we generalize are as complex as this; or at least that, though some objects may be infinitely complex, we sometimes have a finite probability that an object about which we seek to generalize is not infinitely complex."

This is a sufficient, not a necessary, condition for the validity of analogical arguments. It is possible to weaken the condition considerably. It has often been maintained (I have done so myself) that the inductive principle, whatever it may be, cannot be proved by induction, since to do so would be circular. Mr. Keynes shows that this statement is more or less misleading.

"If our conclusion is C and our empirical evidence is E , then, in order to justify inductive methods, our premisses must include, in addition to E , a general hypothesis H such that C/H , the *a priori* probability of our conclusion, has a finite value. The effect of E is to increase the probability of C above the initial *a priori* value. . . . But the method of strengthening C/H by the addition of evidence E is valid quite apart from the particular content of H . If therefore we have another general hypothesis H' and other evidence E' , such that H/H' has a finite value, we can, without being guilty of a circular argument, use evidence E' by the same method as before to strengthen the probability H/H'

"Our assumption, in its most limited form, then, amounts to this, that we have a finite *a priori* probability in favour of the Inductive Hypothesis as to there being some limitation of independent variety . . . in the objects of our generalization. Our experience might have been such as to diminish this probability *a posteriori*. It has, in fact, been such as to increase it. It is because there has been so much repetition and uniformity in our experience that we place great confidence in it. To this extent the popular opinion that Induction depends upon experience for its validity is justified, and does not involve a circular argument."

It seems (though Mr. Keynes does not make this clear) that even in the above restricted form we still have a sufficient condition which has not been shown to be necessary. There may be other hypotheses besides that of finite variety which would justify the inductive method. I have no such hypothesis to suggest, but so far as I can see Mr. Keynes does not prove that none could be found. The possibility of other hypotheses would of course strengthen the case for the validity of induction. Mr. Keynes does not profess to have proved the validity of induction, but only to have discovered a hypothesis which, if it is true, will justify induction. This, I think, he has done. I think, also, that his hypothesis could be deduced from the theory of quanta. But that theory, of course, could not be established without the use of induction.

It is interesting to observe that, if Mr. Keynes is right, the validity of induction as a method of establishing the probability of generalizations depends upon a characteristic of the world which is not logically necessary. The world might be so constructed that induction would always lead us astray, not in the sense that improbable things would happen, but in the sense that the falsehood of inductive generalizations would not be improbable. Mr. Keynes thinks we do not live in such a world; I hope he is right.

I have not space to deal with Part IV., "Some Philosophical Applications of Probability," or competence to appraise Part V., "The Foundations of Statistical Inference." The latter contains much admirable criticism. I wish it had contained some discussion of the statistical parts of physics, such as the second law of thermodynamics. In essence, statistical methods are an extension of induction, and raise no new question of principle. This Mr. Keynes brings out clearly.

Considering the book as a contribution to logic, three points seem specially important: (1) the view that probability is indefinable; (2) the contention that probabilities are not always (or even usually) numerically measurable; (3) the analysis of induction and analogy. Assuming the correctness of Mr. Keynes's views of the first two points, I do not think what he says on the third can be questioned, though it probably can be supplemented by further work founded upon it. But if he were mistaken on the first two points, it might be possible to simplify his theory of induction. His difficulties here arise from the fact that the *a priori* probability of a generalization has to be *finite*, i.e. greater than some numerically measurable probability other than zero. If all probabilities were numerically measurable, this condition would always be fulfilled. I shall not, therefore, further discuss induction, but shall briefly consider the other two points.

If probability is definable, the theory of probability will not be an independent part of logic, but (according to the only definitions that seem plausible) a branch of pure mathematics wholly derivable from the same apparatus from which the rest is derived. The only serious attempt, hitherto, to define probability is the frequency theory. Reduced to its simplest form, this theory states that, given two properties *A* and *B*, when we say that the chance that a thing which has the property *A* has the property *B* is *p*, we mean that *p* is the ratio of the number of things having both the properties *A* and *B* to the number of things having the property *A*. This theory is seen at its best in relation to such a question as: what is the chance that a number less than 100, chosen at random, will be a prime? Mr. Keynes has difficulties with such questions, because pure logic (which must be included in our premisses) enables us to prove that any actual number either is a prime or is not a prime, so that probability only exists before the number is specified. Nevertheless I am satisfied that his arguments against the frequency theory in its crude form are valid. He is less convincing in arguing against a modified form of the theory suggested to him by Dr. Whitehead.* His objections here are not addressed to the principle, but consist in showing technical difficulties which, one feels, might be overcome by ingenuity and skill. I think, at any rate, that there are very strong arguments for the view that probability attaches to a propositional function rather than to a proposition. Thus, in the above instance, the propositional function '*x* is a prime' has a probability-relation to the functional premiss '*x* is a number less than 100,' but '*4* is a prime' has not the same probability-relation to '*4* is a number less than 100.' It may be doubted how far this takes us in the direction of the frequency-theory; but it certainly involves, if true, a not inconsiderable modification of Mr. Keynes's view.

The question whether probability is definable has of course an important bearing on the question whether probabilities are numerically measurable. On the frequency theory, probabilities are fractions. Some other definition may be found, which would enable us to prove either that probabilities are, or that they are not, numerically measurable. But, even if we accept Mr. Keynes's view as to the nature of probability, it may be doubted whether it follows that numerical measurement is sometimes theoretically impossible. Measurement is always largely conventional, and some convention might be invented for comparing probabilities lying on different paths between impossibility and certainty. There is also the following point: Mr. Keynes rejects the view that the probability of a proposition on given premisses can always be compared, as to greater and less, with that of its contradictory;

* I take this opportunity to protest against Mr. Keynes's practice of alluding to *Principia Mathematica* as though I were the sole author. Dr. Whitehead had an equal share in the work, and there is hardly a page in the three volumes which can be attributed to either of us singly.

if such comparison were always theoretically possible, every probability would be greater than, equal to, or less than $\frac{1}{2}$. If, then, p and q were two independent propositions each of which was less likely than its contradictory, the probability of pq would be less than $\frac{1}{4}$. In this way, an indefinite number of numerical approximations would become possible. I do not know whether it can be maintained that the probability of a proposition is always comparable with that of its contradictory, but it seems evident that, where this is not the case, it is not rational to allow the probability of the proposition to influence conduct. In the chapter on "The Application of Probability to Conduct," Mr. Keynes seems to suggest, though he does not definitely assert, that non-numerical probabilities may rationally influence conduct; and elsewhere he suggests that it is rational to give weight to an uncertain induction when no better induction can be ascertained. I cannot see how this can be justified except when the probability of the proposition in question is known (or believed likely) to be greater than that of its contradictory. It seems to me, therefore, by no means certain that probabilities are incapable of numerical measurement (at least approximately), except in the very special cases allowed by Mr. Keynes's use of the principle of indifference. These are, however, only doubts; I have nothing positive to urge as against Mr. Keynes's scepticism.

Much of the criticism in the book is extremely valuable, and the mathematical calculus is astonishingly powerful considering the very restricted premisses which form its foundation. The book as a whole is one which it is impossible to praise too highly, and it is to be hoped that it will stimulate further work on a most important subject which philosophers and logicians have unduly neglected.

BERTRAND RUSSELL.

Weather Prediction by Numerical Process. By LEWIS F. RICHARDSON. xi+231 pp. 4to. Diagrams. 30s. net. 1922. (Cambridge University Press.)

The enormous advances which were made during the last half century in most of the physical sciences were not shared by meteorology. Certain very material accessions to our knowledge of meteorological processes have been gained, it is true; but in view of their importance and the fact that all the various meteorological processes are always at work before our eyes, it must be admitted that the advance was small when compared with the results achieved in some branches of science. And yet a very definite step forward has been made, and it is to be found in the manner in which the meteorologist regards his science. The vast accumulations of systematic observations,* which were regarded as the essence of meteorology not very long ago, to-day occupy but a secondary place. The present day meteorologist seeks the physical explanation of observed phenomena by different means. He realises that statistical analysis alone will not lead far, on account of the complexity of the phenomena caused by the close inter-dependence of all the factors involved. He strives, therefore, to place the science on a mathematical and dynamical basis, and measures his success by the extent to which he is able to achieve this end.

The first step in this direction was taken by Ferrel, who applied to the atmosphere the equations of motion of a fluid on a revolving sphere. The value of this method of attacking meteorological problems was not realised for some time, and it is only comparatively recently that it has been followed up by Shaw, Jeffreys and the late Lord Rayleigh. The dynamics of revolving fluid as represented by a cyclone has attracted the attention of all these investigators, but as Shaw remarks, "the application of equations of revolving fluid to the phenomena of cyclones" is a subject that "is as yet almost unexplored." The first two investigators have shown the necessity of taking account of the small terms in the differential equations, and Shaw has shown

* ["Whether the effect of (printing the observations of many observatories) will be that millions of useless observations will be added to the millions that already exist, or whether something may be expected to result which will lead to a meteorological theory, I cannot hazard a conjecture. This only I believe, that it will be useless at present to attempt a process of mechanical theory; and that all that can be done must be to connect phenomena by laws of induction. But the induction must be carried out by numerous and troublesome trials in different directions, the greater part of which would probably be failures."] SIR GEORGE AIRY, 1867.]

how the distribution of velocity in ordinary cyclones is consistent with their being regarded as simple vortices. But even now we know practically nothing about the mechanism by which cyclones are developed and maintained. These and other problems still await elucidation, and there can be little doubt that their solution will come from a more detailed study of the dynamics of revolving fluid.

But perhaps the most important and the most brilliant of recent contributions to dynamical meteorology is the work of G. I. Taylor on atmospheric turbulence. The phenomena of eddy motion in the atmosphere appeared to be almost hopelessly insoluble, until Taylor showed that the problem could be tackled by dealing with eddies *en masse*, and by examining the effects produced by large numbers of eddies acting over relative long intervals of time. His original conception, according to which momentum, heat and water-vapour are all propagated in the atmosphere by eddies in a similar manner to the conduction of heat in a solid, was developed in order to explain the vertical distribution of temperature over the sea. But its most fertile application has been to the case in which momentum is the entity propagated by eddy diffusion. Taylor himself first used it thus to provide a partial dynamical explanation of the observed vertical distribution of wind velocity and direction.

He also showed that the observed diurnal variation in the wind near the ground follows from a corresponding variation in the amount of atmospheric turbulence. Richardson and others have objected to Taylor's work inasmuch as it contains the assumption that turbulence does not vary with height above the ground. This criticism is now generally conceded, and the determination of the laws of the variation of turbulence with height forms one of the problems on which meteorologists are at present engaged. Here again the mathematical method promises to be at least as effective as the experimental; for the laws of the variation with height of wind velocity and direction—which have since been obtained—can only be compatible with definite corresponding laws for the vertical distribution of atmospheric turbulence.

Yet another meteorological problem which is being attacked mathematically is that of evaporation from water surfaces. The subject is closely connected with atmospheric turbulence and is, moreover, one of extraordinary complexity. Nevertheless, the work of Jeffreys shows that it is, at least partially, amenable to mathematical treatment.

In the above sketch a brief attempt has been made to indicate the important part which mathematics plays in modern meteorology.

The consummation of the mathematical method is to be found in this latest work of Mr. Richardson. The author compares his book with the Nautical Almanac, but the task which he has set himself is in reality a far more complex one. The factors which he endeavours to cope with include the horizontal and vertical movements of the air, the conveyance of water and heat, the effects of latent heat, precipitation, radiation, atmospheric turbulence and evaporation from the sea, from land and from foliage.

Differential equations are developed which represent the contributions of all these factors towards the future weather, and we are then shown how to solve them by the method of finite differences on a set of twenty-three different computing forms. The originality of the whole conception is such as one has learnt to associate with Mr. Richardson's name. This process of forecasting is necessarily extremely laborious, and for this reason alone has little chance of competing with the present empirical method, for the present at least. But the main value of the book lies in the fact that it presents a co-ordinated dynamical treatment of meteorological processes which has not hitherto been attempted. Many investigators have devoted their attention to the dynamics of wind, to the problems arising from atmospheric turbulence, or to the thermodynamics of meteorological processes, but in the present work we see for the first time the true relationship between all these various factors, and how each contributes to that very complicated phenomenon which we call "the weather."

Regarded as a method of forecasting the weather, Mr. Richardson's process suffers from the fact that a local deviation of the wind in the lower layers of the atmosphere is liable to produce grievous errors in the forecast. In Chapter IX.

the author applies his process to an actually observed set of conditions. His success is only partial, and the failure appears to be due to the above cause.

Again, the co-ordinate intervals employed are—for time 6 hours, for latitude 3 degrees, and for longitude 200 km. Small scale phenomena, such as thunderstorms and "local showers" are consequently smoothed out, and the method will "not help us for example to say whether it will hail or not on Mr. X's field."

Throughout the book Mr. Richardson employs consistently a system of notation which is explained in Chapter XII. Like the rest of the book, the notation possesses certain original features. In addition to the English and Greek alphabets, both small and capital letters, we find weird Coptic letters and a number of symbols of the author's own invention. The meaning of each letter and symbol is described both in English and also in the international language Ido. An excellent system is employed for facilitating references.

The book is published by the Cambridge University Press and is a good example of their best work. N. K. J.

The Fourth Dimension. By E. H. NEVILLE. Pp. 55. 5s. net. 1921. (Cambridge University Press).

Professor Neville has fastened upon a fact which should have been obvious but has been seriously overlooked. While many have been talking at length upon Non-Euclidean geometry in four-dimensions, and particularly upon the very complex notions of the curvature of three-dimensional manifolds in four-dimensional space, or of four-dimensional manifolds in five-dimensional space, very few have had any preliminary preparation in the shape of a detailed consideration of the most elementary propositions in four-dimensional Euclidean geometry. The ordinary procedure in everyday geometry is to begin with the study of straight lines and planes, then to proceed to spheres, and so gradually to the differential geometry of surfaces in general. This may or may not turn out to be the proper order in which the systematic study of hyperspace should be undertaken; but at any rate it is worth trying. The first chapters of the text-book are here presented to us.

What are the meanings to be attached to the words "line," "plane," "space," when we come into the world of four-dimensions? This is the question which is answered in detail in the first half of this useful little book. It is shewn how the geometry of four-dimensions is but algebra with the addition of some terminology adopted by analogy with three-dimensional geometry. Unfortunately, the dictionary which Prof. Neville tells us he is writing, is faulty at one point. A line is 1-dimensional, a plane is 2-dimensional, a space is 3-dimensional, but there is no distinctive name for the 4-dimensional domain within which these others may be described; and the adoption of the word "space" for a 3-dimensional variety satisfying one linear equation, brings us into confusion with the term "hyperplane," which has often been used in this sense.

These are small matters, however, and the student who will take the trouble to read these pages attentively will be well rewarded by finding his power of thinking in four-dimensions strengthened and clarified.

The later part of the book dwells particularly on the generalization of the idea of "rotation." In ordinary space the displacement of a rigid body with one point fixed from any one position to any other leaves a certain axis in the body fixed in position. Prof. Neville proves in detail that in four-dimensions the displacement of any system of points, the intervals between each pair being unaltered, and one point being fixed, leaves all the points of a certain plane (2-dimensional manifold) undisturbed. He gives us, in fact, a rational and complete description of the idea which, noticed by Minkowski, was the germ-thought of all the analytic development of the theory of relativity. In 1908 Minkowski pointed out that the Lorentz-Einstein transformation was, formally, precisely a rotation in four-dimensions about a fixed plane. This led him to the unification of space and time into what is now called a space-time. It is greatly to Prof. Neville's credit that he has perceived the need for an elementary exposition of the matter, and that he has had the courage and humility to set his hand to it. We are very grateful to him for doing so.

E. CUNNINGHAM.

Principles of Geometry. Vol. I, Foundations. By H. F. BAKER. Pp. 182. 12s. 1922. (Cambridge University Press.)

This is the first of a series of volumes, the others to appear shortly, on curves and surfaces of the lower degree, up to 3 and 4, and gives "the indispensable logical preliminaries." The outstanding feature of the treatment is that idea of distance (and therefore of congruence) is not introduced, and the whole geometrical system is based on incidence and order. The full justification of this will appear when we see the properties of the commoner geometrical entities flowing as simply from this set of propositions as from the more usual assumptions. But the present volume is also of the highest interest in itself.

A theory equivalent to that of cross ratio and harmonic section is built up from the propositions of incidence, by means of perspective and the complete quadrilateral. It is shown at length that Desargues' Theorem follows from the three-dimensional assumptions, but cannot be proved from the plane propositions only (the proof is obscured by an unfortunate slip on p. 120). At the end of Chapter I., algebraic symbols are introduced, with great care, and the commutative law for multiplication is interpreted in connection with Pappus' Theorem. Chapter II. deals with the ideas of accessible and inaccessible points, based on assumptions of order, and leads very naturally to postulated points, which remove most of the restrictions of this Real Geometry. At the end of Chapter III., we have an elaborate system of sets of real elements, which can, if desired, take the place of imaginary single elements, and so justify the introduction of the latter into a purely geometrical argument. Throughout the book, any number of dimensions are contemplated.

The matter is not well displayed: the main Propositions of Incidence are huddled together in the second paragraph, and not even numbered; and quite a fair amount of eye-strain would have been saved, in reading off joins and cross-joins, by printing on p. 51 for example, $\begin{smallmatrix} A N B' \\ B A' N' \end{smallmatrix}$; it would have been well worth the extra space. On the other hand, we are almost entirely spared footnotes, and references are placed where they should be, in a separate Bibliographical chapter. There is an excellent table of contents and a poor index. We shall look forward with great interest to the later volumes.

H. P. H.

Plane Geometry—An Account of the More Elementary Properties of the Conic Sections, treated by the Methods of Coordinate Geometry, and of Modern Projective Geometry, with Applications to Practical Drawing. By L. B. BENNY, M.A., B.A. (Lond.), F.R.A.S. Pp. 336+vi, with five portraits. 10s. 6d. 1922. (Blackie and Son.)

To meet the needs of beginners and students of average ability many books on elementary analytical geometry have been published during the last twenty years. Here we have a work which attempts to fill a more advanced place. It is avowedly written for examination purposes, and covers the ground of pure and analytical plane geometry required for the London University B.A. and B.Sc., as well as for Part I. of the Cambridge Mathematical Tripos. The book strikes one as keeping a little too closely to the former of these schemes.

It is a difficult matter to write a good mathematical text-book of a pass standard, for the subject is in the nature of things truncated at somewhat arbitrary places, and there is difficulty in making the book a unity, for no part of the work can be rounded off and completed. What actually can be done is to pursue the elementary use of geometrical methods logically and with clearness, as far as the required pass standard admits, to show the relations between the several methods, and further to interest the reader both by suitable references to past history, and by suggestions as to advances beyond the scope of the book. The writer may even, by way of example, dip a little into one of these more advanced lines of enquiry.

Broadly speaking, the author of *Plane Geometry* has succeeded in his aim. He has given a systematic account of straight lines, circles and conics, treated both by the older and newer synthetic methods of pure geometry, and by Cartesian methods. The common bond of all is the method of projection.

which is treated in some detail; and in dealing with the theory of perspective and of homographic ranges the book reaches its most advanced point.

The arrangement and development is clear and distinct; a chapter is devoted as a rule to one method at a time. The choice of theorems is good, for they are not overloaded with details, yet they cover very much ground. There are plenty of appropriate examples throughout, with a good set of answers and occasional hints. All this is evidently the outcome of useful practical experience.

A pleasing feature of the book is the insertion of portraits and short historical notes on Cayley and other famous geometers; but surely the arrangement would be better if these notes came boldly into their natural places in the book instead of at the end. The reference to Descartes would make an excellent addition to the opening of the first chapter, especially if the Greek origins of synthetic methods also received attention somewhere in the text.

As to choice of subject, it is difficult to see why for elementary students so much attention should be paid to cross ratio, projection, and general homographic relations, if at the same time—and this is no accidental omission—there is no mention of involution and reciprocation. To be sure a theory of duality is hinted at on p. 32, but the author must have unduly repressed himself when he exhibits the theorems of Pascal and Brianchon as disconnected phenomena. Again, the book confines itself to plane geometry, and yet for the purpose of definition utilises solid geometry both for projection and for the conic section—quite rightly. But the question at once arises, why not make the book more homogeneous, by bringing in more notions of easy solid geometry and discarding some of the complexities of the later pages? Desargues' Theorem is actually easier to prove for a solid than for a plane figure. This suggests further possibilities. It seems to the reviewer that a hard line of distinction between plane and solid geometry, where one throws light on the other, is as much to be deprecated as the older line now erased between pure and analytical methods.

This criticism is rather strengthened because the book with few exceptions lacks references, internal and external. There is every reason to believe that a student who has worked through this carefully graded book would be enlightened to know that the general equation of the second degree represents a conic, and that certain other treatises have something to say on this subject of conics in general. The above theorem may be deduced by combining three separate results far apart in the book, but this discovery is left to the reader.

There might be improvement in various details: thus there seems to be some confusion of thought in explaining positive and negative numbers for coordinates. Surely the essence of this lies in its economy; we measure in *one* direction only along the axis of x , instead of two; and we succeed in this by postulating positive and negative numbers. The four arrows in Fig. 1, pointing outwards along Ox, Ox', Oy, Oy' , are certainly misleading.

Again, the first great difficulty that beginners have in Coordinate Geometry is to solve a locus problem involving one or more parameters. This has certainly been well provided for in one recent elementary text-book, but is overlooked here.

The definition of a conic section from what its name implies is good, but the classification is very artificial: the *natural* classification depends, from this point of view, on how the plane of the section is situated relatively to the cone rather than its position relative to a circular section of the cone. Nor is it clear whether a right circular or an oblique cone is meant.

The notation $A \rightarrow B$, $a \rightarrow b$ for a vector AB is unwise, as the arrow is more usefully employed in the symbolic expression of a limit.

The book is well printed and invites perusal; the figures are very good, with the exception of one on p. 106, which is unduly baffling. An index would improve the usefulness of the book very considerably. The book should serve its purpose for preparation before examination, but it lacks that breadth of treatment which its scope deserves.

Plane Geometry for Schools. Part II. By T. A. BECKETT, M.A., and F. E. ROBINSON, M.A. Pp. 242-452 + viii + (Answers). 1922. (Rivingtons.)

This well arranged volume is in three sections with appendix, good index, and an abundant supply of varied examples (with answers). It completes

a course of plane geometry of straight lines and circles, introducing the ideas of inversion, cross ratio, and a little trigonometry. The book is compact and very complete, and the authors are not afraid of logical procedure; they always try to make clear their assumptions, e.g. in limiting their proofs to commensurable ratios.

Where it is needed, a page of suggestive commentary is introduced, and there are a few interesting historical notes on ancient problems impossible to be solved by ruler and compass. The authors cautiously 'suppose' this impossibility and do not commit themselves.

Two details seem a little confusing—the use of capital letters P, Q for lengths of straight lines; and the thick type of line in some of the figures, a form of emphasis which is very subjective and does not help every learner.

A good volume on solid geometry overlapping and extending these two Parts would be very useful.

1. **Elementary Algebra.** By C. O. TUCKEY, M.A. Pp. 278 + xi (Contents) + xii (Answers). 6s. 6d. 1921. (Arnold.)

2. **A Short Algebra.** By H. P. SPARLING, M.A. Pp. 120 + vii. 2s. 4d. 1922. (G. Bell and Sons.)

3. **Plane Trigonometry.** By A. DRESDEN, Ph.D. Pp. 110 + iv. 8s. 6d. 1921. (Wiley.)

4. **Mathematics for Students of Agriculture.** By S. E. RASOR. Pp. 290 + vi. 1921. (Macmillan, New York.)

These books are all nicely arranged and, except No. 3, are well printed. They are not so discursive as some of the best recent books on the subjects. The first, which bears no date, would be improved if it had an index.

It is clear that the thought given to improving the teaching of algebra is bearing fruit. We have now a fairly definite aim—to give shape to the idea of the undetermined variable, to generalise arithmetic through formulae to notions of functions and to certain characters or behaviours of certain easy functions. The theory of variation and a few steps in the calculus are brought into a good general course for all students, and the more formal manipulation of symbols, enjoyed by the mathematical boy but not by the majority, is relegated to a special course for the few.

A Short Algebra nearly covers a course for the pass standard of the School Certificate Examination. It includes variation, progressions, and just mentions the binomial theorem, but excludes notions of the calculus. The main idea of the book is to reduce formal manipulation to a minimum and to establish the idea of a function. The writer thinks that the calculus needs a separate volume; he has also for shortness cut out nearly all explanatory matter from the bulk of the book and left nothing but examples. This is unfortunate, for it detracts from its educational value, as it throws the whole onus of teaching upon the teacher. These examples are so exceedingly well graded that, were the needed explanation added quite shortly, many boys in an average class could learn direct from the book.

Elementary Algebra covers the same ground, and includes a little of the calculus and has a section of more difficult aspects of the elementary work for those who intend to go further into algebra. The text explains the steps as it proceeds, and there is careful attention to details which prove stumbling blocks to beginners. Thus the name "Directed Numbers" is given to positive and negative numbers—a useful designation; and the little that is said about these numbers is not misleading. Further on, a logarithmic proof is given that $x^n \rightarrow 0$ if $n \rightarrow \infty$ and if $|x| < 1$.

It is a good plan that, for example, where results can be given without intermediate work, the book supplies a list of answers only to the even numbers of the questions.

For some reason it does not seem necessary to be precise and to define technical terms as methodically as this used to be done. Here, for instance, "terms" are spoken of on p. 29 and defined on p. 34. What was wrong in the older books was to crowd in a heap of unnecessary technicalities; we could

not reach the interior comfortably because the furniture choked the doorway. Now the way is open, but we still like to know what we mean when we talk.

The book is a good straightforward course, without attempt to justify procedure by its history or otherwise.

Plane Trigonometry, which is too expensive for its length, is an attempt to reduce the dull load of trigonometrical detail to a moderate size. Its chief interest is the point of view, namely that in discussing $\sin x$, $\cos x$, and so on, we are dealing with functions of x . So a large place is given to graphs of these functions, and to inverse functions. Projection is early insisted on, and a good economical proof of the addition theorem results. The book emphasises arithmetical trigonometry, and does not go further than solution of triangles.

Whether it would suit beginners at school is open to question, but it promises to be useful for first-year work of ordinary students at the University. It lays good foundations for further advance.

Mathematics for Students of Agriculture. This is a useful manual for those who are interested in the applications of mathematics to the common things around them. The book gives a general course involving arithmetic, algebra, geometry, surveying, mechanics, and includes progressions, logarithms and the binomial theorem. There is variety of illustration and a pleasing lack of irrelevant trivialities.

The book, however, is dogmatic, so that in one sense it is little more than a large collection of relevant formulae with a running commentary on how to use them, but not on how they arise.

The chapters on practical ways of measuring, and especially on land surveying, are more detailed, and the author has something interesting to say on the difficulties which beset the American Congress when in 1785 a law was passed which proposed to divide the extensive non-developable surface of their land into townships six miles square, "as near as may be."

H. W. TURNBULL.

GENERAL TEACHING COMMITTEE.

The Committee met on Saturday, 8th April, at 29 Gordon Square, W.C., and, among other business, passed the following resolutions:

Sequence in Geometry. That in the opinion of the General Teaching Committee of the Mathematical Association it is most undesirable that examining bodies should reduce the freedom of the teacher by imposing an obligatory sequence of propositions in Geometry.

Examination Questions. That a sub-committee be appointed to consider criticisms of mathematical questions set in public examinations.

Letters containing such criticisms should in future be sent to Mr. W. J. Dobbs, 58 Priory Road, South Hampstead, N.W.6.

W. E. PATERSON, *Hon. Secretary*.

Examination Questions Sub-Committee (v. page 83 of the *May Gazette*).—The members of the Sub-Committee appointed to consider criticisms of examinations are: Miss E. Glauert, Scarborough High School; A. W. Siddons, Rendalls, Harrow-on-the Hill; W. J. Dobbs, 58 Priory Road, Hampstead, N.W. 6.

Examination questions on pure or applied mathematics within the school range, if open to criticism, should be sent with comments to one of the above.

W. J. DOBBS, *Hon. Sec.*

ERRATA.

P. 87, vol. xi. Note 626, last line. For "perpendicular" read "parallel."

P. 85, vol. xi. Note 621, line 7 up. Delete "John."

THE LIBRARY.

THE Library has now been removed to 29 Gordon Square, London, W.C. 1, and Mr. W. E. Paterson has taken over the duties of Honorary Librarian.

The following books have been presented to the Library by Mr. J. Brill :

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Select Exercises for Young Proficients in the Mathematicks. By T. SIMPSON, with life of the author by C. HUTTON. 1792.

Essay on Probabilities and on their Application to Life Contingencies and Insurance Offices. By A. DE MORGAN. 1838.

Study and Difficulties of Mathematics : Arithmetic and Algebra : Examples of the Processes of Arithmetic and Algebra. By A. DE MORGAN. (Library of Useful Knowledge. *Mathematics*. Vol. I.) 1836.

Essais sur L'Enseignement en général et sur celui des mathématiques en particulier. 4th edit. By S. F. LACROIX. 1838.

Traité élémentaire d'Arithmétique. By S. F. LACROIX. 1848.

Nouveaux Elémens de Géométrie. (Port Royal.) 1711.

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